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PART FIRST.

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REVIEW I.

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7. *Minutes of Information, collected with reference to Works for the Sewerage and Cleansing of the Sites of Towns.* General Board of Health.—*London,* 1852. (Parliamentary Paper.)

It is quite unnecessary to dwell upon the recent events which have given a new interest to the subject of the zymotic as distinguished from the sporadic diseases. An unusually violent epidemic yellow fever, ravaging a part of the "outlying possessions of this country, and decimating the inhabitants of some of the principal towns in the southern part of the United States; and at the same time the immediate approach of the third visitation of the malignant Cholera to Britain since 1830—are events which may well excite attention, even in the most thoughtless breast, to the laws by which it has pleased Providence to guide these still mysterious agents; and must have suggested to many the important practical reflection, that the study of Nature is adequate to confer great benefits on mankind, even when the powers which we seek to control are apparently beyond our reach, and are hardly susceptible of any modification from all the resources of our art. When the course and effects of a disease are beyond our power, we may still, by a simple induction of facts, ascertain its external causes; and if this knowledge of the laws of nature is allowed the influence which it ought to have on the councils of nations, we may be fully justified in asserting, that we do more for the prevention of sickness and suffering among mankind, by studying the mode of propagation of these diseases, even so far as yet known, and giving that advice by which they may be shunned, than we should do by the discovery of a new remedy, more powerful than any that is known in medicine.

When it is remembered that so late as the time of Sydenham, the greater part of the annual mortality in London resulted from four diseases (plague, small-pox, dysentery, and scurvy), which are still known, and still nearly as fatal as ever to those who are affected by them, but that the number who take these diseases in a given time in this country is now comparatively trifling, simply because their specific causes are known, and may be counteracted,—we have said enough to show, that this *second great object* of medical inquiry and observation is, at the present day, and in this country, a matter of peculiar interest and importance.

Neither can it be said that this department of our science has failed to attract attention, or that the *cupiditas veri videndi* applied to it has been a vain or unprofitable inquiry in the present age. The example, to which we shall afterwards refer, of the *diffuse* or erysipelatous inflammation, the knowledge of its peculiar effects, both local and general, and the different textures of the body which it may affect; and more especially, the careful induction of facts by which it has been put beyond doubt, that, whatever other sources it may have, it is the natural effect of the application to any part of the living body, deprived of its protecting cuticle, of a peculiar *cadaveric poison,* which has been long known to be frequently evolved during the decomposition—we are pretty sure only during a certain stage of the decomposition—of the human body; and the proof that this same poison is the immediate or exciting cause of one of the most distressing kinds of epidemic disease, the puerperal fever;—these constitute a body of information which, imperfect as we must admit it to be, is sufficient, in the great majority of cases in which that malady can present itself, to disarm it of its terrors; not by opposing the diseased action itself, but by enabling us to give advice, by which those who would otherwise be liable to its attacks may be effectually preserved from them. And we shall immediately show, that our information regarding the malaria exciting intermittent and remittent fevers, and likewise regarding the contagious poison exciting continued fever, in this climate, is sufficient, when circumstances admit of its being fully applied and firmly acted on, to preserve from these diseases the great majority of those who must otherwise be their victims. At the risk, therefore, of "tiresome iteration," we shall resume this subject at some length, endeavouring now to fix the attention of our readers rather on what has been ascertained of the natural history of the specific *exciting* causes of some of the most important epidemics, than of the *predisposition* to them, of which we have recently treated.

Several of the principles which we may regard as established in regard to the Etiology of the epidemic and endemic diseases, are indeed of such importance, with a view to the great practical result of prevention of disease, that they may be said to possess the same value in that view as the contrivance called a *catch* has in the actions of a machine, being a point up to which the requisite actions may be depended on, and from which a new series of actions, designed for some special end, may safely and beneficially originate.

Indeed, so much is this the case, that we think it a serious fault in many of the observations which are continually given to the world on this subject by medical observers, that they do not fix with sufficient confidence and expression of certainty on certain principles which we may hold to be established by decisive evidence; and therefore they seem to leave the grounds of the advice which they give as ambiguous and vacillating, when in fact they may state these as invested with the certainty and precision of the exact sciences. We need not say that if there are principles on this subject, which we regard as put beyond farther controversy by the evidence already obtained in support of them, it is doubly incumbent on us, on that account, to reflect carefully on the grounds of those opinions, and be prepared to show that the facts have been accurately observed, and that our inferences have not gone beyond what strict logic will justify.

When we remember that the knowledge acquired by what we call the empirical observation of diseases, and of the effects of various influences, either on their accession or their decline, is greatly in advance of that which we possess regarding their pathology, or intimate nature, we may be prepared to expect that it can only be by observations of individual cases, repeated so frequently as to come under the denomination of Statistics, that we can ascertain the "universality of the facts," which we thus elevate to the rank of principles. It is by the "numerical method," therefore, that these principles must be established; and so much has been said and written in regard to that method of inquiry, that it is worth while to pause for a little, and endeavour to satisfy ourselves as to what may be expected, and what is not to be expected, from the applications of that method in medicine, and particularly in medical police.

First of all, a preliminary error, we believe, of very considerable importance, has been frequently committed in considering this subject, the more dangerous as it comes under the guise of political wisdom and prudence. It has been said that, with a view to the health of communities, as well as to other objects, specific regulations by law, founded on such knowledge as that of the remote causes of disease, among an intelligent people, enjoying a free constitution, are unnecessary or injurious, because such a people will always be disposed to consult their own interest in all their municipal regulations; and that this immediate object of all political communities, if not interfered with by forms or acts of government, may always be reckoned on with as much certainty as in the conduct of an individual; that in the case of at least a community enjoying a good constitutional form of government, true political wisdom consists simply in leaving the regulation of all matters affecting the public health to the good sense of the people, provided only that these shall be duly instructed, and left quite at liberty to follow their own interests.

The *true and general answer to this we believe to be, that if "the true test of the excellence of a constitution is to be found,"* as stated by Professor Stewart,* "in the details of its municipal code," liberty and a good form of government are themselves valuable, with a view to the end of all governments, *ut cives feliciter vivant,* not as a means of good government, but only as a security against bad; and that, truly and essentially useful as we believe that security, in the end, to be, still the experience of mankind, which is more to be trusted than any political opinion, has clearly shown that, at least in the present state of public information, we have no security in the circumstance of political liberty, for regulations affecting the health of a people being either wisely framed, or carefully followed out. Not to mention the deficiencies which may be observed in the regulations for the education of the poor in this country, and again, for the preservation of our people from small pox by universal vaccination, as compared with the state of these matters in some of the despotic states on the Continent,—we may here observe, that in this country, and at the present day, it continually happens that streets are built, edifices of all kinds erected, even towns planned, laid out, and inhabited, with perfect neglect of the simplest precautions for securing ventilation or draining; or promoting, in the simplest ways, the purity of the atmosphere. We need give only two illustrations. The first is supplied by a letter published in the *Times* newspaper of September 30, 1853, describing the condition of a piece of ground in the vicinity of the Hackney-road, which is at the disposal of so benevolent and intelligent a body as the governors of St. Thomas's Hospital. Eight or nine years ago, we are informed—

> "This land was let by the hospital to a speculative builder, for a long term, at a high rent, but without any conditions in reference to drainage. The consequence was, that a large number of houses was speedily built, and to every house was attached a small, badly constructed cesspool; the ground being very retentive, these cesspools are continually in a state of overflow, and the whole soil is completely saturated with the filthy overflowings, the stench from which is at times most sickening and disgusting. If any of the subscribers to the hospital will take the trouble to inspect the rows of houses on the right hand side of Fellow's-street, leading out of the Hackney-road, they will see a state of things which is a disgrace to a civilized country. The only wonder is, that typhus and cholera should ever be absent from a spot so carefully prepared for their reception."

The other case is the town of Merthyr Tydvil, which has been very rapidly extended of late years, in connexion with the iron trade in its vicinity, and which was allowed to attain the population of 37,000 souls, before any one thought of providing a drain for any of its houses; after which time, of course, the construction of drains became an object of much greater expense and difficulty than it would otherwise have been.

If farther illustration of this principle were wanted, we could easily produce it, by referring to the "fetid marshes of Montfauçon, at Paris," or the "Stable Nuisance," lately reported on—in the hope that a favourable opportunity for suppressing it is presented by the present alarm—by the medical profession in Glasgow.

We do not think we go too far in asserting that even in this country (and, *à fortiori,* in most other countries of the world) we can have no security for the interests of the people in regard to health—especially the interests of the poorest of the people, whom we know to be the most liable to disease, especially to epidemic disease, and whose health, with a view to the general interests of the community, is therefore of the greatest importance—being adequately provided for, otherwise than by making them, under certain regulations, the care of the State, and extending to them the protection of the Law.

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* Life of Adam Smith.

In every inquiry, directed with such views, regarding the efficiency of any alleged cause, either of the accession or abatement of a disease, the greatest difficulty will very generally be found to result from the circumstance simply and shortly stated by the late Mr. Playfair, in his 'Commentary on the works of Bacon'—the impossibility in these inquiries of commanding all the conditions of any experiment, or contrived observation, so as to leave out one after another of these conditions in each repetition of the observation, and have an *instantia crucis* as to the influence of any one of them on the result.

> "The *instantia crucis*," he says, "is of such consequence in all matters of induction, that wherever it is unattainable,—a part of the conditions of every observation we may make being fixed by Nature and beyond our control,—there must necessarily be a great want of conclusive reasoning. This holds of *medicine* and of *political economy*. Making two observations exactly alike in every respect but one, is what the *instantia crucis* and the method of induction in general especially requires, but it is what in these sciences, for the reason now given, can hardly ever be accomplished. Men deceive themselves continually in such cases, and think they are reasoning on facts only, when they are in reality reasoning on a mixture of facts and hypotheses."

In most cases, the true and legitimate mode of overcoming this difficulty is simply, to multiply the observations—especially the comparative observations—made on cases to which any alleged cause, either of the accession or abatement of a disease, has been applied, distinguishing as accurately as possible those in which the other conditions of the observation have varied. Although in every one of these some peculiar unperceived conditions may no doubt exist, which are not found in others, and which may affect the result, yet, if the observation is repeated a sufficient number of times, and under all varieties of circumstances which *can* be perceived, it may confidently be expected that these *unperceived* or uncontrollable variations in its conditions—some of them favouring, and others opposing, the effect of the cause which is under trial—will ultimately destroy one another, and the efficacy or inefficacy of that cause, known to have been common to all the cases, will be made manifest.

But although the general principle of multiplication of cases, in order to exclude those antecedents of the event under consideration, which may have escaped our observation or been beyond our power, is that on which we must chiefly rely, in applying the method of induction in many of these inquiries—i. e., we must put our information in the form of *statistics*—yet it is easy to perceive, on a little reflection, that several fallacies must peculiarly embarrass all such applications in medicine. In stating these, we are so far from wishing to set aside this method of inquiry, that we shall very often find it to be only by further application of statistics, consequent on further reflection, and subdivision of the subject, that errors otherwise unavoidable may be corrected, and that we can proceed, in the way of induction or exclusion, until we find ourselves, *in the phraseology which has lately become prevalent among* some of our northern theologians, "*shut up to certain conclusions.*" We set aside the doctrine, *Testimonia ponderanda sunt, non numeranda*, as distinctly inapplicable to many inquiries in medicine, but nevertheless maintain with confidence, that in all cases, *Testimonia ponderanda sunt antequam numeranda.*

I. We may first direct attention to the remarkable difference between such application of statistics to Etiology, and therefore to the prevention of diseases, and to Therapeutics or their treatment. In the former case, the importance of the practical rules which may be thus suggested is very apt to be *underrated*, because the practical result to which we look, on any trial of such rules, is merely *negative*—it is the nonappearance of disease in persons whom we suppose liable to it; and this result, although it may be perfectly well founded in statistics, is not matter of ocular demonstration, and very often makes little impression on the public. In the latter case, the probability is, that the efficacy of the measures under observation will be *overrated*, because the desired result is the *positive* one of recovery of patients; we know, that in almost every case, various causes besides that under trial have contributed to that result; in acute cases, especially, the salutary provisions of nature for the decline of diseases, or, as we may very often more correctly express it, the essentially temporary nature of the diseased action itself; in chronic cases, more remarkably, the unobserved agency of other external circumstances besides the remedy in question. Of the degree in which these causes have contributed to the fortunate event of any individual case, all candid and intelligent medical men will allow that it is very difficult to judge; and without judging of them, we can have no certain inference as to the power of any remedy.

It is, however, pretty generally admitted by medical men, that it is to inquiries in Etiology—i e., into the *causes* of disease and the *means of prevention*—that statistics are really most applicable; but we have not seen so general or satisfactory a statement as we think may be given, of the circumstances of this inquiry, which make it so much more open to the evidence of numbers, than the investigation of the power of remedies.

The questions to which the numerical method is to be applied, in the former case, are generally in reality much simpler and more general than in the latter. Many of the questions of this kind, indeed, even of those on which the rules of medical police or the most useful suggestions for the prevention of disease are founded, belong to that very general class, in which we are not required to inform ourselves of the nature of the diseases which certain influences produce, or fail to produce; the mere amount of sickness and of mortality is enough to establish the propositions in question. When we find that—the average annual mortality in this country being 1 in 45 or 46—the mortality in a particular town or district rises, in any one year, to 1 in 28, or even to 1 in 22; if we are sure that this mortality is fairly ascertained, and that the obvious source of fallacy from immigration and emigration is avoided;—or when we find, that in a particular trade the average duration of life, instead of being 40 or more, is under 30 years, we have the important, although very general principle established, that some of the peculiarities of the mode of life of those among whom such mortality is observed, must be peculiarly prolific of disease. On the other hand, when we have such a statistical statement as that given by the late Dr. Luscombe of the health of a division of the army in Spain, engaged, during a week of almost incessant rain, in an expedition to Arroyo de los Molinos, in which they outmarched and overthrew one of the most active divisions of the French army, passing two of the nights of the week in bivouac without fires, and find that, nevertheless, the number of sick in that division was less during that and the next week, than in any other equal period of the year,—we have in that simple statement evidence, such as nothing but statistics can furnish, of the efficacy of some cause, acting on the constitutions of those men, which must have counteracted the usual effect of cold, wet, and fatigue, in producing disease; and we can hardly conceive that any circumstances common to this large body of men can have had this effect, excepting those to which Dr. Luscombe ascribes it—viz., "exercise and mental excitement."

Even in this simplest case, however, of such inquiries, it has often happened, that the inference at first drawn has been greatly beyond what subsequent experience has justified; and the reason most generally has been, that the conditions of the observation (generally made with a view to the action of some special cause) have been thought to be simpler and more under command than they really were; and the observation has been supposed, therefore, to approach closely to the *instantia crucis*, and to warrant a specific conclusion as to the efficacy of a single cause; whereas farther examination and more minute subdivision of the subject have been necessary, before any statistical principle really of that character could be deduced from it.

Of this we may give an example from the writings of an author whom we highly respect, although we cannot doubt that in this and some other statements relative to the prevention of disease he has fallen into this error. Dr. Southwood Smith observes, in the course of discussions on the effects of air vitiated by decomposing animal or vegetable matter, that the districts of a town which are undrained will very generally be found much more liable to disease, and particularly to epidemic diseases, than those which are drained; from which he infers, that it is by the diffusion through the atmosphere of putrescent matters, which good draining would carry off, that these diseases are produced, or that the poison exciting them acquires what has been called an epidemic influence; and when this statement is taken along with the somewhat hasty assertions, to be found in several scientific works of late years, as to the intimate relation, if not identity, of the process of decomposition of an organized body after death, with the changes effected in a living body by the action of malaria or contagion,—it is not surprising that it should be regarded as an exposition of ascertained truths, very different from what have really been made out.

We have no doubt that the observation itself will be found very generally correct; but in regard to the inference, we must observe, *first*, that the condition of the inhabitants of the undrained parts of a town differs from that of the inhabitants of other parts, in many other circumstances besides the degree of vitiation of the air which they breathe; and, *secondly, that the vitiation of the air which they breathe depends on many other* causes, besides those which draining can remove; it depends on the construction of the houses, and of the streets, courts, or alleys which they inhabit, often such as to make ventilation impossible; it depends especially on the degree of crowding of their rooms, and very often likewise on their own habits: they are very generally the poorest of the artisans, many are, indeed, destitute; they are ill fed, ill clothed, ill lodged, almost always crowded together, and careless as to the use both of pure air and pure water, often inadequately protected from cold, often exposed to fatigue, often addicted to intemperance. It is a fact statistically proved, and more general than any other that has been ascertained in regard to the health of different portions of the human race, that among those who are most in want of the comforts of life, there is the greatest amount of sickness and mortality. In order to ascertain, therefore, by the method of induction, that the unhealthy condition of any poor district of a town, or that the prevalence of any epidemic disease there, is owing to defect of drainage, we must have the subject subdivided, and statistical evidence adduced on its subdivisions, excluding other peculiarities of the condition of the people there, and fixing attention on the results of the deficiency of draining only; which, so far as we know, has not yet been done in this country.

To show the necessity of such subdivision of this inquiry, in order to give to its results that precision which we may hope to acquire by them, we would beg to refer to a paper on the health of the different districts of Paris, published many years ago (in 1825*) by Villermé and Villot, but framed, as we conceive, more strictly on the method of induction or exclusion than any one we can mention—published in the view of maintaining any special doctrine as to the causes of disease,—in this country.

It appears from that paper, that on comparison of the bills of mortality in the different arrondissements of Paris for ten years consecutively, these different districts of the town preserved with remarkable uniformity the same relation to one another in regard to mortality; the average mortality of the whole city being about 1 in 32, the greatest mortality, that of the 10th arrondissement, being about 1 in 24, and the smallest, that of the 1st arrondissement, being about 1 in 40.

These physicians then attempted to ascertain whether this uniformly greater mortality could be ascribed to a denser population in the unhealthy districts—whether estimated by the number of houses to an acre, or by the number of inhabitants to a house; next, whether it could be ascribed to differences of soil, or to vicinity to or distance from the river, or, again, to the fetid marshes of Montfaucon. None of these conditions appeared to influence the average mortality of the districts in which they existed, simply for this reason, that when arranged according to the degree of any one of these conditions, the different districts were found to occupy very different positions from what they did in the order of healthiness already mentioned. But when the different districts were compared according to the numbers of their *pauvres*, i.e., of inhabitants who were excused from payment of taxes on account of certificates of destitution, they were found to arrange themselves, year after year, in the same order as when compared according to their mortality—the 1st arrondissement occupying the lowest place, and the 10th the highest—so that the examination of the records of the mortality in this city serves only to confirm the general principle long understood, and which is laid down by Mr. Chadwick, in his 'General Report on the Sanitary Condition of Britain,' that the probability of life in any class of men is greater, *ceteris paribus,* as their circumstances are more elevated above destitution, and less as they are in nearer proximity to that condition (pp. 155-157).

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* See Archives de Médecine for that year.

Some of the most striking of the results of his inquiry, in districts widely different in other respects, are contained in the following table of average age at death, of

	Gentry and their families.	Tradesmen and Artificers.	Labourers.
Derby	49	38	21
Truro	40	33	28
Manchester	38	20	17
Rutlandshire	52	41	38[*]
Bolton	34	23	18
Bethnal Green	45	26	18
Leeds	44	27	19
Liverpool	35	22	15

So much in regard to the questions as to medical police, and the prevention of disease on a large scale, which are so general and simple, that they do not imply any exercise of the power of distinguishing diseases, in order that they may be answered; we are concerned only with the number of cases of all diseases, and the number of deaths in a given time, as proportioned to a given population; and we wish it only, at present, to be observed, that it is undeniably by the simple process of multiplying cases carefully under the right heads, as belonging to certain districts, and occurring at certain times, and attending to *all* the circumstances of those districts—i.e., by the "numerical method," that the information on those points, which it is the object of our science to acquire, can be obtained; but that in doing so, much caution and circumspection are often requisite, to prevent our inference from extending beyond the data, and restrain the natural tendency of the mind to assign causes such as *we* deem adequate, for phenomena, which we profess only to be enumerating and arranging.

II. In the greater number of the inquiries which come under the head of Etiology, we require, indeed, the exercise of the power of diagnosis, to fix the titles of the diseases to which these inquiries relate, but we are spared all occasion for the more difficult exercise of judgment, which is required of us whenever we have to give an answer to a therapeutical question, as to the efficacy of any remedy in any disease. In almost every case, when such a question presents itself—hardly even excepting the case of a remedy alleged to possess such a specific power as quinine exerts over ague—it is not alleged that the virtue of the remedy will uniformly and unequivocally show itself; it is only said that *if* the remedy be given at the right time, and with the right precautions, the progress of the symptoms constituting the history of the disease may be expected to be modified—that symptoms which seem urgent will abate—

[*] This very small mortality may be partly referred to habitual emigration.

that symptoms to be expected will not appear; or that a tendency towards a particular mode of injurious or fatal termination, which had shown itself, will be altered. It is here obvious that the *judgment of the practitioner* on some of the most delicate questions in prognosis and in practice—as to the dangers to be apprehended, or the time and mode of administration of remedies in individual cases—is *one of the elements* of the question we have to decide; and when we reflect on this, we cannot be surprised to find that we can frequently satisfy ourselves much more completely, as to the alleged efficacy of a new remedy, by watching the progress of a single case of a well-known disease in which it is carefully administered, and the subsequent progress of the symptoms accurately noted, than by studying a number of tables, exhibiting the use which has been made of the remedy in a great number and variety of cases, where we have no security for similar equally careful observations having been made on it—sometimes where we are merely informed of averages that are struck, on a subsequent review of the cases, as to the period of the disease when the use of the remedy was begun, the length to which it was carried, the degree of certain symptoms thought to demand it, or the mortality from disease with or without its use.

We do not mean to deny that questions occur in Therapeutics, likewise, as to which large numbers of cases may be compared with advantage, and the "numerical method" applied; but we think we have said quite enough to show, that many sources of fallacy must necessarily embarrass the application of that method to all questions regarding the cause, or combination of causes, to which we should ascribe the abatement or decline of a disease; and that it is reasonable and right, therefore, for practitioners to build their opinions as to the powers of a remedy on observations of very different kinds, besides the mere enumeration, and statement of ultimate results, of the cases in which it is given; or, as it is shortly and justly expressed by a practical author, that in order to make up our minds as to any such question, it is better in general "*to watch than to count.*"

In order to enable us to judge how far any given disease may be ascribed to the influence of any external cause, the only strictly medical question before us is as to the *diagnosis* of the disease, preceded by such alleged cause; and as we rest nothing on the judgment of the practitioner touching the more difficult questions, in prognosis and practice, which we have stated, we may reasonably admit that merely by the force of numbers we may often have a body of evidence brought to bear on this question, to which it would be unreasonable to refuse assent.

III. In order to perceive this more distinctly, it is necessary to attend to one consideration which we think has not attracted so much attention as it deserves—viz., that in those inquiries into the external causes of diseases, the number of individuals to whom our contrived observations or experiments may easily be made to extend is often very much greater than it ever can be in therapeutics; and therefore, if these observations have been carefully made and accurately recorded, they will very soon acquire, by the mere force of numbers, the full force of the *instantia crucis.* When we wish to inquire, by the help of statistics, into the action of any remedy on a disease, our observations are necessarily limited to those persons in whom that disease exists; but when we are inquiring into its remote causes, it very often happens that a much greater number of persons, who remain unaffected with the disease, form, in fact, the most important part of the experiment; and, according to the principle already stated, therefore, the experiment soon becomes decisive, and the subject one of those to which we would apply the observation already made, and maintain that to refuse assent to the principle thus supported, is to mistake the nature of the evidence which the subject requires, and injuriously to retard the progress of the science.

This is most distinctly perceived when a disease previously unknown, or at least long absent, appears in any country, or district, and the question is, what are the conditions that determine its appearance. The most important observation in such a case very frequently is, not only that a succession of cases of it occurs in a particular locality, or under particular conditions, but that it occurs then, or under those conditions *only*;—i. e., the great mass of the community, who were previously, and remain subsequently, free from the disease, are the most essential part of the experiment by which its mode of extension is ascertained.

A good example of this kind is given by Dr. Davy, in proof of the contagious property of Plague; which is the more important, as he states that this fact carried conviction to his mind, before uncertain from anything he had previously observed in the East, as to that contagious property. And we perfectly agree with Dr. Davy in the observation, that in such an inquiry, where a single case affords any such strong evidence, it is right to fix our attention exclusively on that one case, and on the extent of information which it conveys, and satisfy ourselves completely as to these points, before undertaking a more general investigation of the subject. This fact was simply, that on a vessel aboard which an undoubted case of plague had existed, being taken into quarantine at Constantinople, and two of the officers appointed for the purpose being sent aboard to examine the cargo, both these officers were seized within a few days with symptoms of the plague, of which one died in the lazaretto—the disease having been, for a considerable time previously, and being thereafter, unknown in Constantinople—i. e., in a population of 800,000 souls.*

Now when we attend to this fact, we think it must appear sufficiently obvious that what makes it decisive is not the mere circumstance of the case of plague having occurred aboard that vessel, and succeeded to others previously occurring there, but it is the circumstance of its having occurred there *only*, at that time. and for a considerable period before and after; and that when this is duly considered, it must be allowed to approach as nearly to the *instantia crucis*—i. e, to an observation made on *many* persons placed in similar circumstances, *in every respect but one*—as can reasonably be desired. For without inquiring into the circumstances of the remaining population of Constantinople, we are quite safe in asserting that among them there must have been great numbers, in all other respects equally liable to plague as these officers of police; but we know that there was no other exposed to the influence of this particular cause—intercourse with the sick; the efficacy of which it is our object to ascertain; and finding that no other was affected by the disease, we infer that the efficacy of that particular cause is established by the true method of induction or exclusion. In all such cases we have a large body of *negative evidence* statistically known, although often not formally stated, to support the *positive* evidence, of individual cases following one another in a particular succession. When this negative evidence is clear and extensive, a very few positive facts become decisive as to the operation of an individual cause, because the cases in which all previous conditions except that one have been alike, being almost infinitely numerous, the influence of that one cause is soon brought to the *experimentum crucis.* But when such negative evidence does not exist, to furnish a comparison with the positive evidence, a great deal of labour may be expended on the statistics of a question of this kind, without any practical result.

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* See Notes, &c., on the Ionian Islands and Malta, vol. ii. p. 333.

We submit the following cases for consideration at this time, with the single observation, that if the facts stated in them are, as we believe, carefully examined and truly represented, we consider them equally conclusive, on the very same principle, as to the fact of a certain contagious property being attached to the malignant Cholera of 1832 and 1849.

The first is, of course, known to us only from the report of the French authorities; as to the others, we have personal knowledge.

1. In 1833, the frigate *Melpomene* arrived at Toulon from Lisbon, at which latter place cholera was raging. The *Melpomene* had lost fifteen men before she started, and more than half the crew had been attacked during the voyage. On her arrival at Toulon, where not a single case of cholera existed, the cholera patients were taken into the lazaretto, where four galley-slave attendants, with an inspector, were sent to wait on them. Four ordinary attendants were also sent on board the frigate. One of the latter was immediately attacked, and died in eight hours. On the next day, two others, who likewise died. The fourth was also attacked, but escaped. Of the four *galley-slaves in the lazaretto,* two died on the second day, a third soon afterwards, and the inspector on the fifth. The disease did not spread beyond the precincts of the lazaretto, and *Toulon remained free from it for two years.**

2. In the month of January, 1832, when cholera was prevailing in Musselburgh, but no case had yet originated in Edinburgh or Leith, the whole population of these towns (above 150,000 people) was more completely under observation than it probably ever was before, or is likely to be again: in hourly expectation of the unknown pestilence, to which every one's attention was turned, medical men appointed to each of the police districts, to give assistance to all who had no medical attendants of their own; stations in each district open night and day, where reports were to be made and their assistance could be had, and all the medical men anxious to report their cases immediately on their occurrence. In these circumstances, it is quite certain that the first case which originated in Edinburgh or Leith was that of widow M'Millan, of which an account was published by Dr. Simpson, in his paper on the 'Contagious Propagation of Malignant Cholera,' in the 'Edinburgh Medical and Surgical Journal,' April, 1838; that this woman's son had been in Musselburgh and slept in a house where the cholera was, on the Monday; that on the Wednesday, having returned to Edinburgh, he was seized with vomiting

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* Medical Times, vol. iii. new series, p. 515.

and purging of watery matters, cramps, and the other symptoms of cholera, so as immediately to excite the suspicion of his having been at Musselburgh, and so exposed to the poison, which was at the time denied, but afterwards confessed; that he was nursed and frequently rubbed by her in a small confined room on the Wednesday and Thursday, and recovered under treatment; that she herself, an aged and infirm woman, was never out of her own *close*, during that week, or many months previously; that she took the cholera on the Saturday, and died in ten hours; and that the disease originated in *no other inhabitant of Edinburgh or Leith for at least ten days thereafter.* When these points had been all satisfactorily established by examinations before the sheriff, it appeared to us, as it does still, that on the true principle of induction or exclusion—just as in the case quoted from Dr. Davy, and in that from Toulon—the evidence of this disease having a contagious property was conclusive; by which proposition we mean merely, that it may be excited by intercourse of the healthy with the sick, without pledging ourselves to any opinion on the mode of communication from the one to the other; and that farther statistical evidence than this one case, the only one out of 150,000 persons resident in Edinburgh and Leith, and not going to Musselburgh or to any other affected locality, in whom the disease appeared, during that time, is not necessary to establish that point.

3. In Dr. Simpson's paper, published in 1838, and in the 'Edinburgh Monthly Medical Journal' for the year 1849, several other instances are recorded of the importation of cholera into detached localities in Scotland, where the evidence of its extension by intercourse of the healthy with the sick is exactly of the same kind, and seems to us equally conclusive; we may mention, in particular, the case of the little town of Bathgate, containing nearly 3000 inhabitants, where four cases of cholera, all fatal, occurred in April, 1832, in strangers who had come from infected districts; two cases immediately after, one of them fatal, in women who acted *as nurses* there, and *no other case,* during the whole of that epidemic, in that town, or in any house or village within seven miles or more in any direction. (See 'Edinburgh Medical Journal,' 1838, p. 361.) Again, the case of the little town of Dollar, into which the disease was introduced on the 12th May, 1832, by a young woman a native of Dollar, but who resided and was taken ill at the Devon Iron-works, about four miles off, which had been severely affected. This young woman came to her mother's cottage in Dollar with the disease upon her, was sent back in a cart the next day to the iron-works, where she died that night (the 13th). Her mother, who had never been out of Dollar, was put into a house a little way out of town, previously prepared as a cholera hospital; there she was seized with the disease in the most unequivocal form on the 14th, and died on the 15th; and we can say with certainty, from happening to know that town well, that *no other* case of malignant cholera has ever occurred in the inhabitants of Dollar from that day to this. To the same purpose, we may mention, among the cases of importation recorded in the year 1849, the village of Dalmellington, in Ayrshire, where a man from Kilmarnock, a town then affected with the disease, fell ill on the 24th January, and died on the 25th; a woman who lodged in the same house with him, and who washed his clothes after his death,

took the disease on the 28th, and died on the 29th, and *no other case* had occurred up to the month of June—i.e. above four months thereafter—when the account of these cases, by Dr. Cruickshank, was published in the 'Monthly Journal.' In like manner, we know that the first appearance of cholera, in the year 1849, in any part of Ireland, was in Belfast; that the first person affected there, and who had the disease upon him when he arrived there, came from one of the affected localities in Edinburgh (College-wynd); that he was taken into the workhouse there, and that after him there was a succession of 30 cases *in that workhouse, and there only;* not only before there were any others in Belfast, but *before there were any others in Ireland.**

The evidence here is precisely similar to that afforded by the case quoted from Dr. Davy, but it has been wholly misapprehended by such intelligent authors as the editors of the 'Edinburgh Medical and Surgical Journal,' who, after publishing the cases above quoted from Dr. Simpson, comment upon them as follows:—"Although the fact of succession in the attacks be established, it does not follow that this succession indicates the relation of cause and effect. We are not entitled to infer, when we establish the fact of succession of these two events, that we thereby establish the relation of causation."† This is certainly mistaking entirely the nature of the inference which is drawn from these cases, because we have already stated that what we rely on is, *not* the fact of a succession of cases *having occurred,* among those who had intercourse with the sick on each of these different occasions, but the fact of these successions of cases having occurred *among them only,* and for a great length of time, while great numbers of others, living in the same neighbourhood, have been similarly situated in every respect but that one, and remained unaffected—i. e. it is the *positive* evidence of a few facts supported by the *negative* evidence of a very great number; and this, as we maintain, affords evidence which, if we are to proceed on the principle of induction or exclusion, we *must* regard as decisive.

We regret to say, but it seems to us obvious, that this evidence is equally misapprehended in the following general statement made by the Board of Health; at least, if this statement is meant to apply to the case of Cholera:

> "Persons who see disease only on a small scale, and who observe that within that small circle one attack is often followed by another in the same family, and that by another, and so on, naturally believe that the second case is caused by the first, and the third by the second, and thus refer the whole series of events to contagion. *It is on this description of evidence that the entire structure of quarantine is based.*" (Second Report on Quarantine, p. 132.)

This, we say, is asserting that we trust to the *positive* evidence of a few cases *only,* as determining whether or not a disease is contagious; whereas we have fully explained, that we trust to that positive evidence only when supported by a great body of *negative* evidence, distinctly *excluding* the operation of any other cause; and we maintain further, that when it is so supported, according to the true principles of induction as known since the time of Bacon, we are not only entitled, but *bound* to trust to it.

As to the poison which excites the cholera attaching itself to fomites, and therefore affecting peculiarly those who, without such precautions as the use of lotions or fumigations of chlorine, come into close contact with, or wash the clothes of persons who have had cholera, we may give the following further statistical evidence, just similar to that afforded by the cases above quoted, and, as we maintain, equally conclusive.

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* See M'Cormac—Directions for Management of Cholera, p. 5. Additional evidence is furnished by the late outbreak in Arbroath.—See Chronicle.

† Loc. cit., p. 406.

1. In the year 1832, the cholera prevailed much among the fishermen at the town of Wick, many of whom, at the time of the herring fishery, congregate there from various quarters. Among others, some sailors from the town of Banff took cholera at Wick, and died of it. A boat carrying their clothes soon after came across to Banff, and several women, relatives of the men, but who had not left Banff, were employed to wash these clothes. Three of these women took well-marked cholera immediately, one of them before leaving the boat where she had been thus employed, and one at least died of it; and we were assured by a very intelligent practitioner from Banff, that these were *the only cases* that occurred in the town of Banff, or, indeed, along the south coast of the Moray Firth, in that year, or from that year to the present.

2. The following case, the particulars of which are published in the 'Monthly Journal of Edinburgh,' for August, 1849, was particularly investigated by Dr. Robertson, who acted as physician to the Cholera Hospital, and as secretary to the committee appointed by the Edinburgh colleges to investigate cholera in that year.

On the 3rd of March, a woman belonging to Campbelltown, in Inverness-shire, at least 50 miles from any place where cholera existed, arrived there from Glasgow, bringing with her a box containing blankets and other clothes which had been used by her sister-in-law, who had died of cholera in Glasgow in the end of January. On the 14th of March she washed these blankets and other clothes, and poured the suds into a drain between her house and that of her next-door neighbours, a father and son, who were engaged in thatching their own house, and used for the purpose a quantity of clay lying close to this drain, besides coming into the woman's house and talking to her. On the 16th of March, both father and son were seized with cholera, and both died on the 17th. Between that day and the 1st of April there occurred in that village 39 more cases, and 12 more deaths; with very few exceptions, intercourse with persons previously affected could be traced in all these; but the decisive fact here is, that when the disease under those circumstances broke out at Campbelltown, it existed *nowhere else, nearer than Glasgow.* (Monthly Journal, &c., p. 1010.) Here, as in the other cases, we do not draw our inference merely from there having been a succession of cases *there*, but from there having been this succession of cases *there only*, and *then only*, in a district extending probably not less than 100 miles square, notwithstanding that the epidemic influence had existed in one part or another of Scotland since the previous October, and a very large number of negative observations therefore exist to support the positive one.

In connexion with this remarkable fact, of communication of the disease by the clothes of persons who have had cholera, we cannot help mentioning several cases, in which it appeared to us in the highest degree probable that it was communicated by the clothes of persons (as in the case of Campbelltown above quoted) who had been in contact with cholera cases, but had not themselves taken it, and in several of these cases never did take it. For example; when the disease was not indeed prevailing very generally, but spreading in several localities in Leith in 1848-9, the mother of one of the young medical men there had been confined to the house by a state of general feeble health for some months previously, and was in a state of great anxiety about her son, who was visiting cholera patients, often late at night, on which occasions she had repeatedly sat up long past her usual hour, to receive him. In these circumstances she took the disease unequivocally, and died of it; and we could hear of no other case in the street in which she lived, or, indeed, in that quarter of the town. Soon after, we went 9 miles from Edinburgh to visit a medical man who had taken cholera, and had died before we arrived. This gentleman had been in attendance on a poor woman (the first case seen in his village) who had been travelling from the south, and had taken cholera before entering the village. His sister, who lived with him, in feeble health, and anxious for his safety, had taken the disease two days before he did, and died some hours before him. A still more striking case was that of one of the first families severely affected in Edinburgh in October, 1848; a family of 8, living in a small room in New-street, Canongate, of whom 6 took the disease, and 4 died of it, between the Sunday and Wednesday. It was denied at first that this family had any communication with Newhaven (the place first affected in Scotland, and, as was believed at the time, in consequence of communication with ships from the Baltic), but one of the daughters afterwards admitted that she had walked through the village of Newhaven two evenings before the first of her sisters was affected, although she did not admit having gone into a house there. This girl was the third of her family that actually took the disease; but considering that the disease undoubtedly existed at that time in Newhaven, and that it had not then shown itself in more than *three or four houses in Great Britain since the year* 1833, we cannot consider her communication with Newhaven to have been only an accidental coincidence. And as it is universally admitted that the specific virus exciting puerperal fever has very often been communicated to patients by medical men, who were, and continued, in perfect health, there is nothing unreasonable in the supposition that cholera may be thus transmitted also.

Again, in regard to the affection of nurses who have, of course, close intercourse with patients in cholera, we can give the following statistical facts with perfect certainty:—1. That in the year 1832, at the time when the whole number affected in Edinburgh had amounted to 1 in 1200 of the population, the proportion of the nurses employed in the hospitals, who had taken the disease, was 1 in 5; and 2ndly, that during the epidemic of 1848-9, the number of nurses employed in the cholera hospital in Edinburgh was 18, of which number 5 took the disease, and 3 died of it. At the same time when this disease was going on in this small hospital, a much larger number of nurses (45), living in the Royal Infirmary, only a few paces distant, employed by the same managers and under the same matron and superintendant, paid, fed, and clothed in the same manner, were engaged in similar occupations, relieving one another in like manner, but tending on patients in other diseases (fevers among the rest) not in cholera; and among these, no case of cholera or resembling it occurred. In the Cholera Hospital at Glasgow, 84 nurses were employed, 9 took the disease, and 4 died of it. In all, of 102 nurses 14 were affected, and 7 died. Having the evidence of these statistical facts, on a comparatively small scale, to indicate that this disease may be communicated by intercourse of the healthy with the sick, we should next remember that on a large scale, as has been illustrated by Dr. Holland, Dr. Graves, and others, this disease has always been found to follow the *lines of human intercourse,* and particularly that two facts have been repeatedly observed in regard to it, on such a scale as to entitle us to call them statistical facts, and which seem to indicate that its exciting cause does not often extend to any great distance from living human bodies. 1. That it has been distinctly observed to make its way in opposition to the trade or monsoon winds, and 2, that it is never observed to make its way from one place to another more rapidly than human beings can travel.

When these different facts are duly considered, we maintain that we do not go further than statistical evidence will support us, in asserting that this disease may be propagated by the intercourse of the healthy with the sick, or with something that has been thrown off from the bodies of the sick; that to doubt the truth of this principle, is to betray an "unmanly want of confidence in the clear conclusions of human reason;" and that any regulations for checking the extension of the disease, which proceed on the supposition of its having *no contagious property,* must be held to be essentially faulty.

But agreeably to the principle above laid down, we must be careful not to carry this conclusion too far. It may seem a contradiction to say that a disease can be both contagious and non-contagious, but there is no inconsistency in saying, that the poison exciting a disease may both be propagated by contagion—i. e., by intercourse of the healthy with the sick, and likewise may, either through the atmosphere or in some other way, be diffused, to a certain extent, over the surface of the earth, independently of any such close intercourse. Various theories have been proposed to explain this. That which perhaps it is most important to keep in mind, because it agrees best with an important part of the phenomena of this and other diseases, is that which ascribes it to swarms of microscopic animalculæ, or vegetative germs which are taken into the bodies of persons exposed to them, multiply there, and excite the disease, but are capable of diffusing themselves through the air, and of attaching themselves to, and *rapidly multiplying on,* other matters likewise.* There is no decisive evidence for or against any such theory, and the microscopic evidence, so far as it goes, is clearly against this one; but we should not do justice to the applications of statistics to this subject, if we did not state shortly the evidence we have for the disease on some occasions—perhaps especially in certain places and in the hot climates—extending itself independently of any close intercourse of the healthy with the sick, and requiring, in order that its extension may be checked, other means besides that separation of the sick from the healthy, which is known to be so effectual in the case of continued fever or of plague.

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* See Holland On the Hypothesis of Insect Life, &c., in Medical Notes and Reflections.

In asserting this to be the fact, we do not lay any stress on the merely negative fact often adduced on the subject, that many persons have close intercourse with the sick, and nevertheless escape the disease. This observation has, indeed, been very frequently made by all who have seen much of the disease in any climate; but when it is stated as proof that the disease has no contagious property, the simple answer is, that if it prove anything, it proves too much. Those who have had close intercourse with persons already sick of cholera and escaped, have likewise breathed the same air—they have generally inhabited the same place,—they have, in almost every case, been exposed to the same epidemic influence, *of whatever nature that may be*, and escaped. If their escape proves anything, therefore, it proves that we were mistaken in supposing that the disease belongs to the class of epidemics—i. e., that it depends *on any cause* which is of local and temporary existence only; which inference, being in opposition to the much more general statistical facts, of total absence of the disease from many millions of men and for long series of ages, and rapid occasional extension within narrow limits of time and space, must be held to be erroneous.

This negative fact, however, taken in connexion with such positive facts as have been stated on the other side, is important as indicating one general principle in regard to the imperceptible cause of cholera—that it is liable to most remarkable variations in its influence on the human body on different occasions. This principle is not essentially different from what may be observed in regard to all epidemic diseases; indeed, if they were not liable, more or less, to such variations, they would cease to be epidemics; but the variations of effect are more sudden, more striking, and seem less capable of being reduced to any perceptible law, in the case of cholera, than of any other. Many facts are known, however, which show that they are mainly dependent on two principles already mentioned, and exemplified in the case of other epidemic diseases, although in a less extraordinary degree—viz., alterations of the intensity of the virus itself, and peculiarities of constitution, giving a predisposition to its influence in some persons much more than in others.

But in asserting that the cholera must have a mode of extending itself among mankind, independent of any close personal intercourse of the sick with the healthy, we do not trust to the negative observation above stated, but to this distinct positive observation, that in many instances, when the disease is prevailing extensively, the proportion of cases occurring in certain districts among those who have no known intercourse with the sick, is at least as great as among those who have close and continued intercourse; and further, that the disease appears in certain localities, affecting numerous detached individuals almost simultaneously, while not only neighbouring localities, but the attendants on the sick in these, remain unaffected.

Notwithstanding the distinct evidence above stated of the disease showing a contagious property on many occasions in Scotland, we have ourselves seen many instances, and been assured of many more, in which this immunity of the immediate attendants on the sick, and nearly simultaneous affection of many within a limited district, who had no direct communication with one another, has been strikingly exemplified. The late Dr. Reid, of St. Andrew's, whose accuracy of observation is well known, and who went from Edinburgh to Dumfries, and took charge of one of the districts of that town, at the time when it suffered more than any other town in Scotland from the epidemic cholera, in 1832, expressed himself distinctly as satisfied, as all the medical men in Dumfries at that time were, that those who had close intercourse with the sick at that time in that town, were not affected with the disease in a larger proportion than those who avoided such intercourse; and Dr. Blacklock, of Dumfries, who saw much of the disease when it prevailed again pretty extensively there during the winter of 1848, found that the same remark might be made upon it at that time. The same is stated by several of the medical men who have recorded their observations on cholera in the 'Edinburgh Monthly Journal' in the year 1849.

A more unequivocal statistical proof of this disease having a mode of epidemic extension independent of actual intercourse with the sick, is given by cases on record in India, in which it has affected severely large organized bodies of men in particular localities, and abruptly ceased on their being removed from these, without affecting peculiarly, sometimes without affecting at all, the attendants of the sick, or those who came in contact with the sick, in those other districts; so as to justify the expressive phrase employed by some of the Indian practitioners, that there are "tainted districts," varying in different seasons, to which the disease is confined.

Some of the cases of this kind given by Dr. Hamilton Bell seem to admit of no other interpretation—e. g., the case quoted from the Bengal Official Report, of a detachment of 90 men of the 26th Native Infantry, who halted, on their way to join the main body at Tangor, on the evening of the 11th May, 1818, after an ordinary march, on the banks of a small lake, under a fortress, but on open ground. About midnight the first man sickened, and died in half an hour, and by sunrise 20 men out of the 90 were affected. They were all carried forward in carts and doolies, but by 11 A.M. on the 12th, when they reached their ground, four were dead and two more moribund, and before the end of the week, *every man of this detachment* was in hospital with cholera, or at least some form of bowel complaint. These men were mixed promiscuously with the others who had been stationary at Tangor, but none of these last men were affected.* Here it may be doubted whether these 90 men who were thus affected nearly simultaneously, caught the disease on the banks of that lake, or whether they had imbibed it previously; but it can hardly be supposed that they infected one another, and it is certain that they did not infect others; so that the epidemic must have had a cause independent of contagion.

The most striking exemplification of this principle, of the diffusion of the disease being confined to districts, and often unconnected with intercourse of the sick and healthy, was in the case of the army of Lord Hastings, amounting (including camp-followers) to nearly 100,000 men, where—

"When the disease was at its height, and all business had given way to solicitude for the suffering, the army was moved, on the 13th November, 1817, and after marching above 40 miles, and reaching the high and dry banks of the Bottah, halted on the 19th. The road, it is stated, was strewed with the dead and dying, and the ground of encampment presented the appearance of a field of battle. But here the army got rid of the pestilence, which ceased to be epidemic on the 22nd; so that this crowded camp, carrying thousands of sick along with it, by moving 40 miles in less than ten days, *shook off the disease.*" (Bell on Cholera, p. 76.)

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* Bell on Cholera, p. 77.

A case formerly related in this journal,* to the same purpose, seems free from all objection, and to be a statistical document as satisfactory as can be desired. In this case, one wing of a cavalry regiment just arrived from England, and in high health, ascended the Ganges from Calcutta (where there was no cholera at the time) in boats. At a certain period of the voyage, the troops arrived at a part of the country where cholera prevailed in the villages on the banks of the river, but with which they did not communicate. Here cases of cholera occurred; they were advised to push on rapidly, and after a few days, when they had passed the limits of the existence of the disease on the banks, it ceased to show itself in the boats. But what makes the case peculiarly conclusive is, that the other wing of the regiment followed afterwards by the same mode of conveyance, became "affected with the disease at the same point, and lost it again at the same point;" so that the limits of the tainted district seemed to be as clearly marked out by the voyage of the one detachment as of the other.

And when we state this evidence of the disease having a method of diffusing itself independent of any contact or close intercourse of the sick with the healthy, we should also remember that it is a general fact, in regard to its extension over the earth's surface, that, although following the great lines of human intercourse, it has made its way, not uniformly, indeed (as the case of the Island of St. Louis shows), but very generally, in spite of cordons and quarantine regulations. Indeed, any one who has observed its sudden and capricious outbreaks even on a small scale, in different parts of a large town or district of this climate, must be strongly impressed with the conviction that it must have some way of diffusing itself—whether through the atmosphere, as many suppose, or along the earth's surface, or under the earth (as some have conjectured), and of *multiplying itself at the points which it reaches*—which we can hardly conceive that any restrictions on human intercourse can surely or uniformly counteract.

But having stated the evidence which seems the most decisive on both sides of this disputed question, we may again ask, Why not admit that the disease can extend itself, and that we ought to be prepared with means of limiting its extension,—in two distinct ways;—one simple and easily understood, the other, more obscure, but equally ascertained and probably even more destructive? Is it not well ascertained, in regard to other diseases, that they may be excited and even spread epidemically, under certain circumstances, both by contagion or the more unequivocal process of inoculation, and independently of it? As to the erysipelas and the dysentery, we have seen cases which enable us to answer this question in the affirmative, to our own satisfaction, and from personal observation. As to the yellow fever, we have repeatedly said that this appears the most reasonable explanation of the facts, which were fully ascertained by Dr.

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* Vol. ii. p. 70.

M'William, as having occurred at Boa Vista. And although we know nothing of the intimate nature of the influence producing the cholera, yet if we put together the facts which have been statistically ascertained on both sides of this disputed question of contagion, and others, perhaps equally important, which are admitted on both sides, have we not obtained an amount of knowledge relative to the laws which regulate its extension,—not, indeed, complete or satisfactory, but highly important, and from which practical rules will emerge, according to the circumstances of individual cases, more truly useful than specific directions founded on more limited views of its causes?

The peculiar virus which produces this disease is known to us only by its effects on the human body, just as electricity or galvanism, or even caloric, is known to us only by its effect on other bodies; and it is by statistical evidence, therefore, that we have acquired all the information we possess concerning it. But in this manner it is known to vary remarkably in its own intensity at different times and places, to be altered by certain agents, and to affect some persons much more than others. We know that it has hitherto been more intense and general in hot climates and seasons than in cold; that it has appeared on different occasions to be influenced by electricity, having abated considerably in effect after a thunderstorm; that it is influenced by moisture of the atmosphere, producing its effect most remarkably in low and moist situations; that it is influenced, like other epidemics, to a certain degree, and its effect increased, by vitiation of the atmosphere from other causes, such as decomposing organic matters. We know that there are certain towns and districts which show a peculiar, unexplained liability to it, being affected severely in different epidemics, as, e.g., Dumfries in Scotland; while others have shown as remarkable an exemption, such as Birmingham; and this fact may lead us to suspect that, besides mere elevation (which has, however, a greater effect), there is something, not yet ascertained, in particular soils, which favours the development of the disease; above everything else, we know that it is influenced, like other epidemics, and more rapidly than any other, by *time*, the disease being always most virulent when it first appears in any town or district, gradually abating in intensity, as every epidemic affecting either vegetables or animals sooner or later does, and ultimately dying out. We know, further, that it is aided remarkably in its effect on the human body by various concurrent causes, which may frequently be avoided,—by exposure to cold, wet, fatigue, imperfect nourishment, especially by disorder of the stomach or bowels, and by intoxication; we know that it takes effect most easily on those in a feeble state of health, remarkably on women during pregnancy and lactation, and in those in whom organic internal disease exists; and yet not on those in whom such disease exists as keeps up a febrile action—few persons affected with tubercular disease, at least of the lungs, falling victims to cholera. We know also, from such facts as have been stated, that, in this country at least, it arises frequently from the human body when affected by the disease; but that it may not only be conveyed, somehow, to a considerable distance from that origin, but *multiply and increase in energy*, in certain situations when so removed from living bodies (in which respect it would not seem to differ from that peculiar matter, of low organization, which produces the aphthæ, nor from that cadaveric poison which excites the erysipelas, or the puerperal fever; and may probably, as we shall see that this last pretty certainly may, be counteracted by the use of chlorine); we know that it may attach itself to clothes and goods, and there multiply and increase in virulence. And to this we add a curious fact (although one which is known only by negative observations, and therefore not advanced with absolute confidence), that it does not attach itself to the *dead body*, at least to the body in a certain state of decomposition; for it is certain that the dissecting rooms in Edinburgh were supplied during the greater part of 1848-9, as they were in the year 1832, almost exclusively by cholera subjects, and in neither year was there a single case of the disease among the numerous students attending these rooms.

But what we think especially important to be observed is, that this virus, whatever its origin, shows a remarkable tendency to attach itself *to places.* There are *tainted districts*, in this country as well as in India, in which, even when epidemic, the disease is observed to almost exclusively prevail; and this leads to what we believe to be most important in the way of prevention. The strong representations made by the Board of Health in this country, of there being a peculiar liability to this disease in damp, filthy, and ill-drained situations, are, we believe, quite true, but they are certainly not the whole truth. The "tainted districts" which we have seen have been often less peculiar in their nature, and always much more *limited in extent,* than could have been supposed from these representations.

Thus, in the case of a family, one of the first affected in Scotland in 1849, several of whom were taken into the hospital in 1849, from New-street, Canongate, and in which there were 6 cases and 4 deaths within four days, the tainted district in which these cases appeared was a *single room,* on the *second* floor of a common stair, which leads to several winding passages and to *many* rooms, all ill-aired and dirty, and most of them having windows which open into a small court containing often receptacles of dung. But it is quite certain, that neither at that time nor since has any case occurred in any other of these rooms. Cholera appeared likewise at that time in Burt's Close, in the Grassmarket, where it might be supposed to be owing to the same circumstances of dirt and deficient ventilation, which had attracted notice when that close was on several occasions a "*nest of fevers;*" but on inquiry it appeared that all the cases of cholera were from a different tenement from any of those which had formerly furnished the fever cases. It was said, indeed, at the time, that the tainted district in this close was connected with the Greyfriars Churchyard, on which it is quite true that the windows of the rooms that were affected open; but it is equally true that the windows of two other suites of rooms in the *lower* stories of the same common stair open on that churchyard, and yet that in these there was not a single case of cholera. In this as in other cases as to cholera, as formerly in regard to fever, we have seen the tainted districts confined to the upper floors of lofty tenements, while the lower, notwithstanding dirt and defective drainage, have remained free from the disease. We have seen it prevail and spread on the very pinnacle of the Old Town of Edinburgh, the highest story of the highest houses on the Castle Hill, while the lower stories of the same were exempt. At the Water of Leith in 1832, and again in the spring of 1849, it appeared in one of the situations in which it might be expected, in a damp and dirty row of houses on the *left* bank of the river; but in several villages, almost exactly similarly situated, a little higher up on the same river, it never appeared; and when it broke out again in July 1849, at Water of Leith, the tainted district was confined to the *right* bank of the river; on examination of which it then appeared, that in point of dirt and defective drainage this portion of the village had long been at least as favourably circumstanced for the disease as the other, notwithstanding which it was perfectly unaffected, both in 1832 and in the main epidemic of 1849.

Another curious fact, which seems well ascertained in regard to this virus is, that those very limited districts in which it has once shown itself certainly continue liable to outbreaks of it for a considerable time, insomuch that if measures of purification are employed, and the disease does not reappear, there is a strong presumption of these measures having been really useful. We have seen more than one place where it reappeared in the same room where it had been two months before, the inhabitants having changed; and two places in which a few decided cases appeared as late as October, 1833, just a twelvemonth after it had existed in the same houses previously. In the town of Hawick, which suffered severely by epidemic cholera (imported, as was believed at the time, from Newcastle) in January and February, 1832, there was a sudden fresh burst of the disease in October of the same year, four cases occurring on the same day at four distinct parts of the town, but all of them in houses which had suffered before, eight months previously. And in this and other cases of fresh outbreaks of the disease, the agency of auxiliary or concurrent causes might be distinctly observed, all the persons now affected having been exposed to cold, wet, and fatigue on digging-up potatoes, on the day before their attack; and, in other cases, the occurrence of attacks of cholera in houses long previously affected was distinctly traced to intoxication.

From this tendency of the disease to localize itself *within narrow limits*, and at the same time, from the admitted agency of some such concurrent causes in exciting it, arises the obvious importance of the measure which was adopted in Edinburgh as early as February, 1832, and which has been recommended by the Board of Health in London during the present year, that of establishing Houses of Refuge, into which the persons from those tainted districts apparently the most liable to the disease, might be received, immediately on the violent attack, or the death or removal to hospital of the first cases, where those persons might be regularly fed, and preserved from cold, wet, and fatigue, and from the use of strong liquors, at least until the time was over when the most rapid successions of cases have been observed to occur, and until the rooms where the disease had broke out had been cleaned, fumigated with chlorine, and thoroughly aired. In the houses of refuge they would of course be kept under observation, and any attack of bowel complaint be immediately met by medical treatment. Of course this resource would only be availed of by a portion of the people in any such district, but we have known different cases where it was availed of by workmen in regular employment, with their families; and it would suggest similar measures to others having the means of taking such steps themselves; and we may conclude these observations on cholera with a brief statement of the results which followed these measures in Edinburgh, both in 1832 and 1849.

In the first three months of the epidemic of 1832 at Edinburgh (which was always very partial in its attacks), the number taken into these houses of refuge was 353 from about 70 houses in Edinburgh and the Water of Leith, 248 of them from this last village, all of them not only from tainted districts, but *tainted rooms*, in which decided cases, most generally deaths had occurred. Of these 353, 15 took the disease, and 7 died after removal to these houses; the remaining 346 escaped. How many of those would have been affected if they had remained in the tainted districts can, of course, only be conjectured; but certainly it was the general fact, that when the disease appeared in the houses of the poor, and no such measures were adopted, several cases, and even several deaths, occurred in almost every house, particularly at the beginning of the epidemic; and at the Water of Leith, where, in 1832, it broke out suddenly and violently, but where the removal of the people was most satisfactorily effected, we had the satisfaction of finding that only in one-fifth of the families where the disease showed itself was there a second case; and what was still more striking, there was only one family in that village in which two deaths occurred, and that was a family which had refused the offer of removal to the house of refuge. In the later period of that epidemic, we can only speak with certainty of what happened in a small district of the town, in which the disease broke out in seven different houses; the inmates of these, not affected within the first twenty-four hours, were all got into the refuge, and the disease went no farther in any one case.

During the last epidemic in Edinburgh, the same system was adopted, but not carried to the extent that was desirable, chiefly on this account, that the chief sufferers by this, as by all other epidemics, were the poor Irish wandering in search of work; many of whom, when offered a refuge for their families in a large building belonging to the city poorhouse, immediately took up the notion that they were to be sent back to Ireland, then so unfortunately suffering from famine, and not only refused to avail themselves of the offer, but sometimes even concealed the disease, rather than run that risk. Into that building at the city poorhouse, however, 196 persons, all from rooms where cholera had broke out, were admitted and retained during the residence of their relatives in the cholera hospital, or till their death, and until their rooms were cleaned. Of these only 5 took the disease, and 2 died while in the refuge; and we could not learn that any of these took the disease after returning home.

ain, a similar refuge was established at the poorhouse belonging to the West Church parish, into which, however, only 43 persons could be induced to go. Of these, one only was affected with cholera, immediately on admission.

Again, in Glasgow during the epidemic of spring 1849, 401 persons (not more, for the same reason as in Edinburgh) were admitted to the house of refuge established in Glasgow, of whom 19 took the disease and 5 died.

Thus, in all, we have a record on which we can depend, of above 1000 persons in these two cities, all of them from *rooms* in which the disease existed, taken into these houses of refuge, of whom 40 took the disease and 15 died, while 978 escaped with their lives. At Oxford, in 1849, the same expedient was tried with 70 persons, and no case appeared among them. Of the results to be expected in any district fitted for the diffusion of the disease, where no such means of removing the population exist, we may judge from the statement of Dr. Hamilton, of Falkirk, of the result of his observations in that town and neighbourhood, where about 400 cases occurred in the winter of 1848-9. He says that he had known 47 instances of two affected in a family, 23 of three in a family, 3 of four in a family, 6 of five in a family, 4 of six in a family, 2 of seven, and 1 of eight in a family; in all, therefore, 251 cases in 86 families; but wherever, he says, he could effect isolation and due ventilation, there was *no second attack* in a house. (See 'Edinburgh Monthly Journal,' May, 1849.)

The numbers above given are somewhat different from those published by the Board of Health, in their 'General Report on Cholera in 1848-9' (pp. 124 and seq.), but we have much satisfaction in observing that the practical conclusion of that Board, as to the importance of such houses of refuge when cholera becomes epidemic, being much greater than that of hospitals for the treatment of the disease, is quite in accordance with our own observations; and is supported by the statement, that "in 1691 inmates of the houses of refuge, of whom they had accounts, there were only 33 attacks and 10 deaths."

This measure of removing the still healthy inmates of a district that has been tainted by cholera, is, in fact, exactly similar to the plan usually adopted in India, of moving troops and moving the inhabitants of villages into more healthy situations, when cholera shows itself among them, and may be more easily carried into effect here, because, as we have stated, the tainted districts are generally so limited.

We trust it will not be supposed that we mean to say anything in disparagement of the important measures for maintaining, as far as possible, the purity of the air of towns and of populous districts, on which so much stress has been laid of late years, and which we believe to be one of the most essential means that can be adopted for improving the general health and strength of a population, and diminishing their tendency to this and other epidemics. But when *the specific poison of cholera is actually among us*, we are confident that the result of experience and of statistical observation is to show, that it must be expected to attack certain persons, and attach itself to certain places, for which we can assign no such reason; and that in addition to these general means for improving the general health, means ought to be taken to remove as many as possible of the unaffected inhabitants from the limited *tainted districts;* and likewise to treat the persons actually affected in these districts, and their clothes and bedding, just as in the case of typhus fever, on the belief that the disease may *probably*, although it will not *necessarily*, show more or less of a distinctly contagious property.

(To be continued.) *W. P. Alison.*

REVIEW II.

Sammandrag af Officieller Rapporter om Cholerafarsoten i Sverge, ar 1850.
Af Dr. FR. TH. BERG.—*Stockholm,* 1851. P. A. Norstedt & Söner.
Analysis of the Official Reports on Cholera in Sweden, in the Year 1850.
By Dr. FR. TH. BERG.—*Stockholm.* 8vo, pp. 370.

A COMPLETE description of the progress of cholera in any portion of our globe, is as yet a desideratum in medical literature; but if the reports collected in 1848, 1849, and 1850 be compared with those obtained of its first terrible invasion, twenty years ago, it will be acknowledged that considerable progress has been made in accumulating evidence, and in examining the bearings thereof on the great questions of quarantine, contagion, and sanitary reform. Amid the hurry, tumult, and anxiety which pervaded all classes when the cholera first appeared in England, in 1831, it would have been difficult, even had we then possessed an efficient Board of Health, to have collected any well ascertained and connected series of facts relative to the mode of propagation of the disease. The invasion of the disorder was in many instances so rapid, and its consequences so terrible, that all attention was directed to escaping its immediate influence, while the time of those qualified to observe was wholly occupied with the treatment of individual cases, and no leisure was afforded to the exhausted practitioner for investigating accurately the mode in which the pestilence first broke out, or for elucidating the numerous questions regarding it which will directly suggest themselves to every well-informed mind. When cholera had ceased, and had passed on to ravage other countries, the contest between the contagionists and the non-contagionists gained fresh vigour, and a virulent controversy was the result; till, as years rolled on, and party spirit wore away, the facts that had then been accumulated were subjected to more sober and impartial analysis; and, on the second invasion of the malady, much more care seems to have been bestowed on the investigation of the all-important question of the contagious or non-contagious nature of the disease. On the side of the contagionists appeared the names of Drs. Copland, Watson, Graves of Dublin, and J. Y. Simpson of Edinburgh; and to the latter two eminent contributors to medical science we owe a series of well-ascertained facts and cases tending strongly to support their views. Some writers there were who seemed inclined towards a *juste milieu* on this much disputed question: they admitted that cholera might, under certain circumstances, be occasionally transported from one person to another, but they maintained that quite as often, and perhaps more frequently, it arose spontaneously, and, like influenza or other epidemics, did not depend for its progress on any contagious property. When the second invasion of cholera occurred, the influence of non-contagious opinions at head-quarters was made evident by the fact, that the disease was pronounced to be incommunicable from one person to another; and it is evident that it was regarded by the then existing Board of Health as a malady arising in a great measure from local influences. We acknowledge to have formerly held the opinion just alluded to; but the facts that forced themselves on our observation during the last invasion of the disorder,* brought with them such strong proofs of the contagious character of cholera, that our ideas underwent a total change, and the evidence that we have since collected and examined has tended only to confirm these impressions. We saw repeated instances where a previously healthy locality was infected by persons arriving from a distance, and from places where cholera then prevailed; we found that these individuals were sometimes apparently in good health when they arrived, or, perhaps, they already exhibited the premonitory symptoms of the malady; we met with them labouring under the disease in low lodging-houses, from whence the disorder spread to the other inmates of the same house or room, and we traced the malady from these lodging-houses to other localities, which, in their turn, became focuses of infection in previously healthy districts. We met with cases, too, which inclined us to believe that cholera could not only be transported by the persons, but likewise by the clothes and bedding of those labouring under the disease; and on some occasions the clothes &c. were carried to considerable distances before being opened out or used; yet they, too, seemed capable of spreading the disorder.

It has been constantly urged by the opponents of the contagion of cholera, that this disease pursues a certain steady course across a country or district, such as has been observed to occur in the case of an epidemic of influenza. We think that Dr. Graves has fairly exposed the utter fallacy of this opinion; he has shown that cholera varies most signally in the directions it pursues. From India, after its first appearance in 1817, cholera spread to Sumatra, China, Borneo, and the Islands of the Eastern Archipelago, while at the same time it radiated towards the north and west, following the great lines of communication towards the frontiers of Europe. As early as the year 1823, cholera was on the borders of the Russian empire in the vicinity of Astracan; but a strict quarantine was then enforced by the Russian government, and the pestilence, diverted from its famous "north-west course," spread in a south-westerly direction towards Arabia and Asia Minor, following the track of the caravans to Mecca, and causing a most frightful mortality in the holy city of the Prophet.†

Cholera appeared at St. Petersburgh in 1831, and was from Russia transported into Poland during the war of Polish independence. From Poland the pestilence advanced into Prussia, and traversed North Germany to Hamburgh; and in November, 1831, it broke out in Sunderland. Sweden and Norway were not affected with the disease till 1834, and yet, if cholera had kept true to its north-west course, it should have reached these countries in 1831, instead of turning to the south-west to ravage Poland and Germany. Following this supposed line, it should thence have extended itself to the northern parts of Scotland, to the Orkneys, Shetland, and Ferro Isles, and to the distant regions of Iceland, all of which countries, in a sanitary point of view, were eminently favourable to the progress of the epidemic. Yet all these northern islands escaped the disease, strict quarantine was enforced, and, though cases of cholera occurred on board of ships in Bressay Sound and elsewhere, the malady never reached the land.

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* That is, the invasion of 1848-9. This review was written some months ago.—*Editor*.

† See Dr. Graves' Essay on the Progress of Asiatic Cholera, Dublin Journal of Medical Science, January 1840, p. 355.

We have stated that no complete report of the progress of cholera through any of the great kingdoms of Europe has as yet appeared; but the work now before us, though necessarily still imperfect, goes far to remove this reproach from the kingdom of Sweden. No exertion seems to have been spared to render the history of the pestilence complete; and the whole of the reports are investigated by Dr. Berg in a spirit of candour and impartiality, which imparts additional value to the facts that they contain. We find here, not only a history of the progress of the disorder through the various districts which it visited, but a detailed account of the preventive measures that were adopted, both on the sea-coast and in the towns and parishes of the interior of Sweden, with reports of the prevalence of other disorders, epidemic or contagious, before, during, or after the invasion of the pestilence, and also tables of the condition of the atmosphere &c. during the same period. The sanitary state of those provinces that escaped altogether is likewise recorded; and the comparative spread of the malady in 1834 is briefly noticed. A valuable table, too, is given, showing the various foreign ports trading with Sweden that were, from time to time, declared to be *infected*, *suspected*, or *free*, by the Swedish Board of Health; though it must not be forgotten that many ports were really suffering from cholera, to a considerable extent, before they were declared to be infected by the Swedish authorities.

The government regulations to prevent the introduction of cholera into Sweden were restricted chiefly to the sea-ports, and to the great lines of traffic through the interior of the country. The more inland frontiers, and those more remote from the great roads, seem not to have been subjected to any very strict supervision, as it was left to their respective authorities to determine whether quarantine should be enforced or not. The prudence of such an arrangement would have been very doubtful if the Swedish government had decided on regarding the malady as contagious; but as isolation was adopted by some parishes, and neglected or entirely repudiated by others, ample means for comparison of the two systems were in this way afforded. Quarantine on vessels arriving from infected ports seems to have been pretty rigidly enforced; at all, or nearly all, the quarantine stations along the coast, instances occurred of vessels arriving with cholera on board; but by strict seclusion from intercourse with the shore, the disease was in almost all instances arrested. These observations, however, apply only to the ports along the Baltic; the great commerce of Gothenburg, and the constant communication with Denmark, rendered the enforcement of quarantine very difficult, if not impossible, on the western coast of Sweden. In this last invasion of the disease, Stockholm and all the eastern ports but one escaped entirely, though, in 1848, the cholera had raged at Cronstadt, and along the Russian coast. In 1834, Stockholm and Gothenburg suffered most severely, while the town of Malmö, where the disease first showed itself in 1850, entirely escaped on the former occasion.*

Malmö, a town of 12,000 inhabitants, is situated on the south-western coast of Sweden, nearly opposite to Copenhagen. It lies well exposed to the sea breezes, but its situation is low, and there are some muddy ditches in the lowest parts of the town, which in hot weather exhale odours agreeable to none but to the olfactories of the anti-contagionists.

* In 1834 the port of Skänor, distant only a few miles from Malmö, was ravaged by cholera.

Malmö is one of the ports of Sweden which has the most frequent communication with Germany; here the steamboats from Lübeck and Travemünde land their passengers, and an almost constant intercourse is kept up with Copenhagen.

The general health of the town had been favourable during the preceding summer, which, until the month of July, had been remarkably cool, but with the commencement of that month hot weather set in, and continued till the end of August, when an unusually low temperature supervened, followed by heavy rains in September. In 1834, when Malmö escaped cholera altogether, a much higher temperature prevailed; and in those central parts of Sweden which remained free from cholera in 1850, the range of the thermometer was much higher than on the coast. We here quote Dr. Berg's own words on the subject.

> "It appears, therefore, from the reports, that a high temperature prevailed all over Sweden during the dog-days (*rotmanaden*), and was succeeded by a sudden and unusual degree of cold, and that this change likewise occurred in those districts where the malady first appeared, but that central Sweden, which almost entirely escaped the cholera, was subjected to a greater degree of heat than the most southern provinces.
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> "It is the unanimous opinion of the medical practitioners of Malmö, that neither the sudden change from heat to cold, nor the cold, rainy, and stormy weather of the end of August and of the beginning of September, nor yet the milder temperature of October, produced any perceptible influence on the progress or intensity of the disease." (p. 58.)

The disorder seems to have broken out in Malmö about the 12th or 13th of August, 1850. Out of 12,981 inhabitants, 1138 were attacked, and 378 died, during the twelve weeks that the pestilence prevailed. The mode in which the malady was introduced does not seem to have been satisfactorily ascertained. A suspicious case occurred in the person of a teacher of languages, named Nordlin, as early as the 3rd of August; but the first instance where the disease was pronounced to be cholera, was observed in the person of a female, Maria Jönsson, who had attended Nordlin in his illness, and two or three days after (August 6th) was seized with well-marked symptoms of cholera, was carried to the hospital, and there died. On the 9th of August, the day that she was removed to the hospital, her child, a boy of two years and a half old, was attacked, and died the next day.

Although the origin of the disease in Malmö cannot with certainty be ascertained, still there are strong grounds for the belief, that the malady was brought by the steamboat *Malmö* from Lubeck, where cholera then prevailed. A son of the cook on board of the above-named steamer died of cholera an hour before the boat left for Malmö. No cases of cholera, however, occurred on board the steamer, nor was it proved that any communication took place between Nordlin or the others who were first affected and the vessel in question. The boat arrived at Malmö on the 27th of July, and two Custom-house officers, who had kept watch on board during the stay of the steamer in the harbour, were seized with cholera and died. We doubt much, however, if the infection in this case can be traced to the stay of these men on board the steamer, for they were not attacked with cholera till the 14th and 16th of August respectively. The disease, however, had already appeared in the dwelling of the Custom-house officer, Jacobson, on the 11th of August, where a female, Mamsell Wred, sickened, and died on the following day. On the 13th Jacobson's wife was attacked, and died on the same day. On the 14th Jacobson himself became affected with cholera, but he lingered till the 23rd. Two of his children sickened also on the 14th, and both died within 48 hours. On the 15th, 16th, and 17th, the two remaining children were seized with cholera, as also a servant-girl in the house, but all recovered. The other Custom-house officer, J. Hansson, was attacked on the 16th, and died that day; and on the following day another officer, C. Hansson, and his wife, fell victims to the malady. Both the Hanssons *had attended their friend Jacobson during his illness.* The child of Maria Jönsson which sickened on the 9th of August, as before stated, was carried to the workhouse and died there on the 10th, on which day the first case of cholera appeared in the above-named institution, when 29 cases rapidly followed, 25 of which were fatal. The disorder showed here, as elsewhere, a disposition to locate itself in certain houses, where almost every inhabitant was attacked. We observe, too, in the report given by Dr. Gräh, that out of those employed about the sick, many were attacked with cholera. Out of three physicians, two died; of eight male attendants thus affected, five were carried off, as were likewise five nurses, out of nine who suffered from cholera. The principal mortality occurred during the first three weeks, but it diminished when better sanitary measures were adopted in the hospital.

It must be confessed that the evidences of contagious propagation above adduced are not of the lightest character, but we are not inclined to deny the influence of other predisposing causes, as of poverty and uncleanliness, in favouring the progress of the malady.

The disease was in Malmö, as elsewhere, chiefly confined to the most wretched and insalubrious parts of the town, and especially to a low-lying quarter called "Bethlehem," inhabited by the labouring classes, and adjoining two swamps with a filthy ditch running through them, filled with stagnant water. In this quarter the mortality of those attacked was above 80 per cent., and from 8 to 19 cases occurred in many of the single houses or tenements. It was remarked that the workmen who provided their own meals suffered more than those whose food was prepared for them in the different establishments where they laboured. Those who had the comfort of a warm meal once or twice a day were less affected than the lower class of labourers, who lived chiefly on salted herrings, sour rye bread, and still sourer milk.

In the villages and hamlets around Malmö several cases of cholera were observed, almost all of which occurred in persons who a few days before had visited those sick of the disease in that town. Thus, in Hyllie parish, a widow died of cholera on the 21st of August, who on the 19th had visited her sick brother-in-law in Malmö. At the farm of Tagarp, half a Swedish mile NNE. of Malmö, a servant of the name of Nils Pehrsson was seized with cholera on the 4th of September, after having been in Malmö on the preceding day. This man recovered, as did also his friend, Nils Hansson, who had not been in Malmö, but had sat for some time with Pehrsson on the day of his return from the town, and who was likewise seized with the disorder the next day. In the village of Sunnana, three-quarters of a mile east from Malmö, Hans Tufveson was attacked with cholera on the 2nd of October, eight days after having been in that town. He recovered, but on the 10th October, both his daughters, aged respectively eight and nine years, were seized with the malady, and the youngest died on the 13th October. In almost all cases, the houses where such patients died were carefully secluded, and the disease did not spread farther.

In the district of Flädie, a mile and three-quarters (Swedish)* north of Malmö, there resided, in a small cottage near the sea shore, a peasant named Ake Andersson, with his wife and three children. On the 27th August the brother-in-law of this man came from Malmö, as his wife had been that day seized with cholera, and had been carried to the hospital. He himself, on reaching Andersson's cottage at Flädie, with his little girl, aged 4 years, laid himself down to rest in Andersson's bed, but rose from thence when the latter came in from his work. On getting-up he vomited, but ascribed this to some brandy which he had taken, and he remained during the night in the house. Next morning the authorities ordered him to return to Malmö with his daughter, but on the road thither the symptoms of cholera became more and more developed, and he expired that evening. Andersson himself had not been in Malmö for fourteen days before this; but he sickened of cholera on the 30th of August, and died on the 1st of September. Within a Swedish mile and a half of Malmö lies the town or city of Lund, containing about 6000 inhabitants. During the warm months of July and of the early part of August, numerous cases of diarrhœa occurred in Lund, but not more than are usually observed in hot seasons, succeeded by sudden lowering of the temperature. It was determined by the authorities of Lund, supported, we presume, by the professors of the University there, that the town should be closed to all persons from infected places on the 22nd August; and this regulation was strictly enforced until the 4th of November of the same year. Ten days' quarantine was imposed on all travellers from suspected localities, and on the 2nd of September all importation of goods or wares from Malmö was strictly prohibited. The great object of these strict regulations was to prevent the influx of the terrified fugitives from Malmö; the duty of watching the entrances of the city was performed by 700 or 800 of the inhabitants, day and night; and passengers arriving in carriages from Malmö were made to change horses outside of the city, and to pursue their journey without entering its precincts. The disease appears to have reached within half a Swedish mile of Lund on two sides, but no case of cholera occurred within the city, and as time wore on, the confidence of the inhabitants in their measures of precaution continued to increase. Other towns and villages, such as Ystadt and Trelleborg, adopted the like precautions, and with the same fortunate results. The district physician, Dr. Ström, reports that these measures of seclusion and quarantine towards suspected persons were in these places warmly seconded by the inhabitants, so that their doors were kept rigidly closed to the fugitives from Malmö; and he remarks, too, that wherever quarantine was adequately enforced, cholera did not appear, and the reliance of the inhabitants on its efficacy was never diminished.

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* The Swedish mile is equal to 11,700 English yards, or 6·640 British statute miles.

Five and a half Swedish miles to the north of Malmö, and half a mile from Helsingborg, lies the fishing village of Raa, with 920 inhabitants. Relying on the fact of this village having escaped the cholera in 1834, the authorities of the place took no precautions to prevent daily communication with Malmö, and many of the fishermen, after having visited cholera patients in that town, and resided in infected houses, were allowed, without hindrance, to return to Raa. From the 18th of August to the 9th of October, 1850, 70 cases and 33 deaths occurred in this village. The disorder attained its greatest height about the fourth week, and its virulence was augmented rather than diminished during cold and windy weather. Here, as elsewhere, a succession of cases frequently occurred in a single dwelling-house.

The first case of cholera at Raa was observed in a fisherman of intemperate habits, who had been at Malmö on the 15th of August, but continued in perfect health up to the 19th, when severe symptoms of the disease set-in, and he died on the following day. Attempts were now made to shut up the dwelling of this man, but, as Dr. Stenkula, the reporter, informs us, crowds of curious peasants had flocked around his bed to witness his dying struggles. The well-known fact that this man had, by way of bravado, handled the bodies of persons dead of cholera in Malmö, no doubt increased the excitement in Raa regarding this case. On the 27th of August the two daughters, and on the 29th the widow, of this first victim, were attacked with cholera, but all subsequently recovered. David Johansson, aged 60, a fisherman of Raa, was seized with cholera on the 24th of August, and died on the 26th. His companion and attendant, Jon Eliason, died on the same day, his brother on the 2nd of September, and his brother's wife and daughter on the 5th of that month. The observations of Dr. Stenkula on these and some other cases are worthy of notice.

> "As regards the contagious or non-contagious character of the disease, I have no hesitation in declaring, in opposition to the generality of the more recent authorities on the subject, that the cholera is essentially a miasmatic contagious disorder. It is true that here, as everywhere else in Sweden, diarrhœa, vomiting, and gastric disorders had occurred, but the appearance of this malady in a locality so healthy as Raa, introduced, as it certainly appears to have been, by communication with an already infected locality, is a fact so well established, that it cannot be disposed of." (p. 82.)

Dr. Stenkula even maintains that the disease can be conveyed by individuals who themselves escape, and instances a case in Raa, where two children took the disorder from their father, after his return from Malmö, while the latter never exhibited any symptoms of the disease. He, however, fully agrees with the London and Christiania reports, in believing that all seclusion of healthy districts is unnecessary, save as respects ships arriving from infected ports. The advantages, however, of shutting up and watching the houses in which the disease may have broken out, and of subjecting the inmates of such houses to close observation and seclusion for a time, he thinks cannot be denied.

In 1834 the island of Gottland escaped entirely; in 1850 about half a dozen cases occurred there, and that only in one part of the island, in the village of Kapellshamn, at its north-eastern extremity. These cases do not seem to have been accurately observed, and consequently we cannot place absolute reliance on them, but it is a significant fact that a sloop from the infected port of Lübeck arrived at Kapellshamn, on the 4th of August, having during the voyage lost one of her crew, from well-marked cholera, while another man on board had taken the disease, but recovered. This death was falsely represented to the authorities of Gottland as having been caused by apoplexy, and after some delay, the crew were permitted to land. The first case of cholera occurred in the children of a tailor (Lindgren), who, on the 13th of August, visited the sloop, and took from thence some clothes home with him to repair. On the 16th two of his children, aged 13 and 9, were attacked and died, and his wife was likewise affected, but recovered.

From the coloured chart which accompanies Dr. Berg's Report, we see that the disease in Sweden did not spread after the manner of an influenza epidemic over the whole face of the country, but that it followed the great lines of communication into the interior, or else showed itself in detached localities upon the coast. Thus, 98 cases and 43 deaths are reported from the village of Rönneby, on the southern coast of Sweden, between Carlshamn and Karlskrona, but neither of these towns were affected, and the latter escaped also in 1834, when, as in 1850, the authorities of the place enforced the strictest quarantine regulations.

Pursuing our course northward, along the western coast of Sweden, we come to the small town of Falkenberg, where a few instances of death from cholera were observed. Nearer to Götheborg lies the district of Fjäräs, 24 English miles south of Götheborg, and here cholera seems to have been introduced by a sailor from that town, who left Götheborg on the 20th of September (the day on which cholera broke out there), and who was attacked with the disease on the 21st, and died on the 23rd. On the 25th, the wife of this man was likewise attacked, and died on the same day. The house was then shut up, and carefully guarded, and no further cases occurred. Other instances of the introduction of the disorder from Götheborg were subsequently observed, but in all cases the same precautionary measures were adopted, and with the like fortunate results. Peasants who had visited Götheborg, where cholera was then raging, returned home in apparently good health, but a day or two after, they were seized with cholera, which extended to those, and to those only, who had communication with them. The district physician, Dr. Carlsson, says—

> "We found these precautionary measures (the closing and watching of infected houses) of great use, for the disease never spread out of houses thus secluded, and when it did show itself in the neighbourhood, it could be traced to those who had visited the infected localities. Here, therefore, the non-contagious character of the disease was not maintained, for those who died in this place of cholera, were exactly the individuals who had themselves visited the infected places, or had been in communication with persons just returned from these." (p. 121.)

At the mouth of the Götha Elv, the great channel of water communication across Sweden to Stockholm, lies the large town of Götheborg—a considerable town, at least for Sweden, for it contains 21,000 inhabitants. In 1834, this busy trading and manufacturing town was most severely visited by the pestilence, which carried off seventeen hundred of the inhabitants. In 1850, the cases were fewer, but the comparative mortality was not much less. 1316 were attacked, and 529 died. Of these 311 were males, and 218 females. Previous to the outbreak of the pestilence, the general health of the town is reported to have been good. When the sudden change from oppressively hot to cold and windy weather took place, in August, 1850, diarrhœa and colicky pains in the bowels became frequent, but these diminished remarkably before the first cases of cholera appeared, about the 22nd of September. Scarlet fever, which had prevailed more or less throughout the summer, was not in any way arrested or influenced by the cholera. Among the individuals attacked were 22 nurses, of whom 6 died. The disease attained its greatest intensity in the third week. Carefully-compiled tables are given in Dr. Berg's Report, exhibiting the range of temperature and the barometrical indications during the whole period that the disorder prevailed in the town, but they indicate no coincidences between the atmospheric vicissitudes and the progress of the pestilence. In Götheborg, as elsewhere, the disease prevailed chiefly in the poorest and the most densely populated quarters of the town, and the intemperate were its first victims, but at a later period even those of more orderly lives did not escape. In a town of such great commercial activity, and situated on the great highway from Western Europe to Stockholm and the east of Sweden, no measures of seclusion or quarantine could be adequately enforced. The interruption of trade by a strict quarantine, and the consequent loss of employment to thousands of the working classes, would have brought them to the brink of starvation, and would have rendered them ready victims to the pestilence, which sooner or later would have made its way into the city with the great crowd of travellers that could not be arrested or turned aside. The authorities of Götheborg did not even consider it possible to prevent the importation of goods from infected places into the town. Here, as in the large towns, the faculty were divided on the question of contagion or non-contagion, but though the exact mode in which the disease was introduced cannot be ascertained, it made its first appearance in those quarters of the town which had the most frequent communication with strangers, and with vessels arriving from foreign ports. It was in the streets along the Götha Elv that cholera first showed itself, among the boatmen and sailors employed about the shipping on the river. From thence it extended to the suburbs, and overstepping the boundaries of these, it broke out in various villages north and east and south of Götheborg. In most of these the disorder did not show itself till some days or even weeks had elapsed from the time it had established itself in the town, and in the majority of instances it was observed first in those persons who had recently visited Götheborg. But its chief intensity seems to have been exhibited in the parishes that border the Götha Elv, from Götheborg to the Falls of Trollhätte. Along this great river there is a considerable population, almost all of whom are employed about the ships and steamboats that pass up and down towards the Great Wener lake and the interior of Sweden. We have not room to make extracts from the elaborate reports here presented to us on these districts, but their main features are the same as those we have before detailed regarding other parts of Sweden. The malady first showed itself in persons who had recently been in

Götheborg, or who, in other places, had been in communication with infected individuals, or at least had worked in the ships coming from Götheborg up the river. Again and again do we meet with instances where an individual returned from an infected spot to a previously healthy locality, and there first exhibiting the symptoms of the disease, communicated it to the other inhabitants of the district. In at least twenty or thirty cases the persons who were first seized with the disorder had been in Götheborg only a day or two before; they were in apparently perfect health when they returned home, but after the lapse of 48 hours, more or less, the symptoms of the malady appeared. It was to the other inmates of these houses, or to those who came from a distance and volunteered their services as attendants, that the cholera first spread.

Thus, in Thorsby parish, Anna Johannisdotter (aged 24), slept one night in Götheborg, in a bed from which the body of one of her relatives who had died of cholera had just been removed. On the following day (November 3) she was attacked with cholera on her way home, and died on the 6th. Only one case of cholera had previously been observed in this parish, and that on the 27th of September, in the person of a sailor who had been brought home sick from Götheborg. After Anna Johannisdotter had arrived at home, and before her death, her grandfather, Jöns Hero, aged 81, was seized with vomiting and diarrhœa, and died on the 8th of November. Anna Hero, his wife, also 81 years of age, died from the same symptoms on the 10th of November, and her case was certified to be one of Asiatic cholera, by the district physician who saw her. Andreas Bengtsson, a widower, of 70 years of age, had read prayers by the bedside of the first-named patient; he was attacked with cholera on the 8th, and died on the 10th of November. No comment on such an array of evidence is required. In this parish no precautionary measures were adopted; the local authorities declared "that the disease would spread no farther than God permitted, and that all seclusion was unavailing." Perhaps, too, another reason was more powerful than this pious fatalism—viz., that the prosperity of the inhabitants mainly depended on their intercourse with the shipping on the Götha Elv, and that any quarantine regulations would therefore deprive them of the means of subsistence.

Some of the best illustrations of the introduction of the pestilence may be found in the report of its progress through the various islands that stud the coast of Sweden.

On the 10th of November, a boatman, already labouring under cholera, returned from Götheborg to Lofoen in the *Skärgärd;* he recovered, but his wife and mother both took the disease and died. The persons who placed the deceased in their coffins carried the malady to another island, Brattö, and from thence it passed to the mainland adjoining. The parish of Ucklum remained free from cholera till the end of October, when a labourer, Nils Magnusson, returned home from the Saetter, or hill pastures at Ström, where several persons had already died of the disorder. Nils recovered, but four persons in his house took the disease, of whom two died. A young woman who attended on these last, sickened and died, and was shortly after followed by her father, who had nursed her in her illness. A labourer, who lived in the house of the last named person, died at a cottage about 6 English miles off, when the cottager next took the disease, and died on the same day. It is observed in the Report, that none but those who had communication with the infected suffered from cholera. No precautionary measures were adopted in this district, or in the greater part of the neighbourhood of Götheborg.

The island of Tjörn is separated from the mainland of Sweden by a narrow sound. Great alarm was felt by the inhabitants when the disease appeared at Götheborg, but their measures of precaution seem to have been lamentably deficient.

The reports of the district physician, Dr. Ossbahr, plainly show that the inhabitants of Tjörn really exposed themselves in every way to the pestilence, and that they were in such a condition, as regards their customs and their habits, as pre-eminently to favour the progress of cholera. The mortality in Tjörn seems to have much exceeded the usual average. Out of 51 cases 20 deaths are recorded.

In Aseby, where the disorder first appeared, the greater part of the inhabitants were attacked, and only three recovered. It was observed too on the other farms, that most of the cases occurred in one or two houses or families. The excessive mortality in Aseby is ascribed by Dr. Ossbahr to the circumstances that three families, which had before inhabited separate chambers, all, upon occasion of the first death from cholera, crowded themselves into a single room, wherein children and adults, the diseased and the healthy, continued to reside in the most extreme misery and filth. Moreover, numerous relatives from other farms flocked in to see them, and many of these becoming infected were carried to their own houses, and were the means of spreading the disease in other places. So excessive, however, was the fear of the pestilence among the people of Tjörn, that at first, continues Dr. Ossbahr—

> "It was impossible for me to obtain attendants to wait upon the sick, except among their nearest relations. At length I succeeded in persuading a few, both men and women, to undertake this office; but, alas! after a few days, the best and the most active nurse, Anna Olsdotter, took the disease, and, still worse, she died!!
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> "When I first arrived on the island (November 30th), I earnestly entreated the authorities not to permit the healthy and the diseased to remain in the same chamber. This, however, was neglected, or no measures were taken to enforce obedience. During the first days of my residence in Tjörn, the doors of the infected houses were constantly closed to me, nor could I anywhere obtain the requisite attendance on the sick; but no inhabitant ever hesitated to attend the funerals of the cholera victims, where brandy, with camphor dissolved therein, was swallowed in immense quantities." (p. 194.)

As to the mode in which the disorder was introduced, Dr. Ossbahr's report is at variance with that of the local authorities. The latter assert that not one out of at least 100 individuals who visited Gothenborg, while the cholera raged in that town, were affected with the disease either there or on their return to Tjörn. Dr. Ossbahr tells us a very different story, without, however, directly contradicting the above assertion:

> "From what I ascertained on Tjörn, a man of the name of Rutger Jonasson, a son of Jonas Pehrsson, in Aseby, had visited Götheborg on the 6th of November, and had there purchased various articles of clothing which had belonged to persons

there dead of cholera. These clothes were made up into a bundle by Jonasson, and were brought by him direct to Aseby, where he placed the bundle in a chest, and allowed it to remain there for eight or ten days. One day, when a number of his relatives were assembled at Aseby, the bundle was taken out by Jonasson, with the remark that, 'the things had now lain-by so long that there could be no danger of infection,' and he accordingly offered the articles of clothing for sale. The clothes were handled and examined by those present, and purchased by some. The day after (November 16th), Jacob Christiausson, a man of 76 years of age, and one of those who had been present at Jonasson's house on the above occasion, was attacked with cholera, and one after another, all who had been there on the 15th, to the number of six individuals, fell victims to the disorder." (p. 195.)

The parish of Quille lies 30 or 40 English miles to the north of Tjörn, and no case of cholera had occurred in the intervening district. The disease here broke out on the 17th of November, shortly after the arrival of three boatmen from the infected district of Wenersborg.

In Lilla Edet, on the left bank of the Götha Elv, about 120 cases occurred, nearly one-half of which proved fatal. Here, as in other places, it was remarked that many persons whose bowels had been habitually constipated for years, became perfectly regular in their evacuations during the prevalence of cholera, but when the pestilence ceased, their bowels again became inactive.

Of four nurses employed at Lilla Edet, two were affected with cholera. The disease was perhaps more widely spread in this district, from the circumstance of its inhabitants being mainly employed on the "Ströms" Canal, which is cut through the rocks from the Wener Lake to the Gotha River, to avoid the Falls of Trollhätta. The amount of commerce on the Ströms Canal may be inferred from the fact, that from the 20th of September to the 29th of October, 1850, 562 vessels passed through the sluices at Trollhätta, and in many of these, in their voyage up the river from Götheborg, cholera had appeared. The people of Lilla Edet were therefore in constant communication with ships and with individuals coming from the infected district of Götheborg.

In the parish of Asbräcka, the first case of cholera was that of Peter Andersson, who had visited his brother, Andrew, in the parish of Fuxerna (8 or 10 English miles off), while the latter was labouring under the disease. Peter Andersson sickened in his brother's house, was brought home to his own cottage, and there died. Five more of his household took the disease, of whom three died. Of his whole family the widow alone survived.

At the Falls of Trollhätta there is a considerable population of 1400 to 1500 souls. Here 96 cases of cholera occurred with 43 deaths. The causes of this high mortality are best given in the words of the report:

"Unhealthy and crowded dwellings, want and intemperance, greatly increased the number of victims, and it was observed that the malady was peculiarly severe in those families where many individuals resided in one or two small rooms; while, when cholera did appear among the better classes, it seldom spread to the rest of the household, especially where the dwellings were large, airy, and well kept. Two old persons, man and wife, who lived about an English mile from Stafvered, but were not known to have had any communication with infected persons, were attacked with the disease almost at the same hour, and both soon died. A woman, aged 30, who attended them, took ill two days after at her own house, but gradually recovered; while her aged parents, residing in the same dwelling, fell victims to cholera after about a day's indisposition. A boy of 6 years of age lived also in the same room, but on the death of the old people and the illness of their daughter, he was sent home to his father, who resided in a wretched cottage at Stafvered. On the day of his arrival there he sickened and died; directly afterwards a girl in the same house was attacked and soon expired; and two children were likewise affected, but recovered. Many of the nurses who tended patients in some of the more remote and more wretched tenements, were carried off by cholera." (p. 219.)

The first case of cholera in Skepplands parish occurred in the person of Anders Jonasson, who, on the 29th of September, had visited Götheborg, to bring home his son Andrew, who was working there. On the 1st of October, this son was attacked with diarrhœa, vomiting, and cramps. In the night of the 3rd of October, the father was seized with the like symptoms, and died on the 4th, at two in the morning. Another son, Hans, sickened on the 6th, and died on the following day.

At Wenersborg, a few miles above the Falls of Trollhätta, the Götha Elv flows out of the great inland lake of the Wener-see. The tide of commerce from Götheborg, which has hitherto been confined to the limits of the Götha river, here spreads out to the various ports situated on this vast expanse of waters. One of these ports is the town of Amal, containing 1297 inhabitants, and situated on the northern shores of the Wener lake. Here 58 cases occurred, but only 12 deaths, which may be explained by the fact, that the disorder prevailed a good deal among the better classes.

On the 7th of October, a ship arrived from Götheborg, in which cholera had broke out four days after leaving that town. We cannot, however, trace the introduction of the disease to this vessel, or to the fact of one of the victims of the pestilence having been buried in the churchyard of Amal. On the 13th of October, the steam-boat, *Arvika,* arrived from Wenersborg (an infected port), and the passengers are then said to have been in good health, but on board of this boat were a number of workmen returning to their homes far up the country beyond Amal, and many of these men, as we shall presently have occasion to relate, carried the pestilence with them to their own distant dwellings. Between thirty and forty miles to the east of Götheborg lies the parish of Möne, where a few cholera cases occurred, while the intervening district for at least 20 miles enjoyed perfect immunity from the disease. The first case was that of Gustav Johansson, aged 25, and an intemperate man, who, on the 9th of November, returned from Götheborg, after having purchased there the clothes of some of the cholera victims. He was seized with cholera on the 11th, but recovered. On the 20th of November, his father, and shortly his mother and two young relatives, were attacked. During their illness they were attended for two days by a cottager's wife, Maria Andersdotter, who sickened in their house with the same symptoms, and being carried home to her own dwelling, died there on the 26th. A soldier's wife, Annicka Winberg, nursed the last-named patient for two days, and died of cholera on the same day. On the 28th, her husband was carried off by cholera, and on that day also his son, who, after attending his mother's funeral, had returned to his own dwelling at some distance, and there was seized with the usual symptoms. His wife, his child, and an old man who frequented his house, were likewise severely affected. A woman, Sarah Blix, who had come from a distance of two miles to wash and clothe the bodies of Annicka Winberg and others, sickened on the 27th of November at her own home. She was attended for two days by her daughter-in-law, who became also affected with cholera, but both eventually recovered.

In the next parish of Timmelhed, the following cases were observed. Johannes Andersson, aged 36, sickened on the 22nd November, after having visited Götheborg on the previous day. On the 29th, his wife sickened, and died after 18 hours' illness. The same evening, his mother-in-law, and also a child of a year old, were attacked, and both died on the following day. On the 2nd of December his daughter, Maja, was seized with cholera, and died on the 6th; and a girl in the house suffered from the same symptoms, but recovered. The house was now shut up and watched, and the disease did not spread farther.

We have now traced the course of the pestilence along the Götha river upwards from Götheborg, to where it debouches from the Wener lake. Some of the traffic from Götheborg passes into the interior of Sweden, to the east of the Wener lake, by the Götha canal, which connects that lake with the Wettern see. The Götha canal joins the Wener-see at Sjötorp. From the 20th of September to the 9th of October, 36 vessels passed the sluices of the canal here, and proceeded to the eastward. All these vessels were from the infected districts of Götheborg or Wenersborg, and few or no precautions against the importation of the disease seem to have been taken. The first case occurred on the 9th of October, in the person of a labourer in the dockyards at Sjötorp. He died after 14 hours' illness; his brother, who attended him, sickened on the same day, and died on the 11th. The brother-in-law of the first-named victim attended the post-mortem of his relative, and took ill immediately after, and died on the 12th. A few hours after, his daughter was attacked, and she died on the 13th. The individuals who attended on these patients all suffered more or less from sickness and diarrhœa, but the disease did not spread farther. The clothes &c. of the dead were carefully fumigated, and the house was avoided by the neighbours.

Cholera also appeared in an isolated spot in Wanga parish, about 15 E. miles due south of Lidkjöping, on the Wener-see. Here 46 cases occurred, with 18 deaths. The first case was that of Gustaf Gabrielsson, who had been to Götheborg on the 12th of November to sell poultry. He was taken ill of cholera on the 17th, but eventually recovered. A soldier, aged 33, who had conversed with this man in the open air, after his return, but had not visited his house, took the disease on the 22nd of November, and before the 28th of that month two of this man's children died of cholera, while another child and his wife were severely affected. On the 27th of November, the nurse who attended the above was attacked, and died on the 4th of December. During her illness, five of her children took the disorder, of whom three died. On the 2nd of December, the nurse who had succeeded the above in attendance on the soldier's family, sickened, and on the 4th of that month two of her children took the disease, and one only recovered. The village where these cases were observed was, as regards its sanitary condition, eminently favourable to the progress of cholera. Large dunghills were accumulated around the cottages, the inhabitants were crowded together in dark and filthy chambers, to which fresh air never found access; while their diet was little, if at all, superior to that of the Irish peasantry. They seem, too, to have been a rude and uncultivated race, for the Report goes on to state, that one afternoon the peasants forcibly entered the hospital, with the view of expelling all the inmates, and this for the sole reason that their maintenance occasioned a heavy expenditure to the parish!

On the north side of the Wener lake, on a promontory in the district of Näs, lies the parish of Eskilsaetter. Into this isolated district cholera seems to have been introduced by the yacht *Anna Maria*, which sailed from Lidkjöping on the 14th of October, after having taken on board several labourers, who had come thither from the infected districts on the Götha-Elv. During the voyage across the Wener lake, two of the passengers became affected with diarrhœa and vomiting; and on the same afternoon, the skipper of the vessel, Nils Olsson, was seized with the same symptoms, and died on the 17th, on an uninhabited island off the coast. On the 19th of October, another of the crew, Gustav Carlsson, was similarly affected, but he so far recovered as to be able to reach his own home at Getterud in Eskilsaetter parish, where he was visited on the 20th by Dr. Ekegreen, who pronounced his case to be one of genuine Asiatic cholera. This man recovered, but on the 21st, his mother, who had attended upon him, was seized with cholera, and died on the morning of the 23rd. A young woman, who had likewise acted as a nurse to Carlsson, was affected with the usual symptoms, but recovered. The clothes of the captain of the yacht were taken to his home at Gaperhult, and shortly after, a young woman in the house where they had been deposited was seized with cholera. A fresh crew was now put on board of the ill-fated vessel, and she sailed from thence towards Carlstadt; but on the 2nd of November, the new captain died of well-developed cholera. The clothes of Nils Olsson at Gaperhult were now carefully fumigated, and the house was closed; the same was done at Getterud, and the disease did not spread farther. Dr. Ekegreen remarks—

> "Whatever differences of opinion there may have been regarding the efficacy of measures for *shutting out* (*utestängande*) the cholera, all seem to be agreed as to the advantage of *shutting in* (*innestängande*) the disease when it has made its appearance in a house, whether in a village or in the country." (p. 296.).

To the north of the Wener lake, and thirty or forty English miles inland from Amal, lies the secluded district of Fryxdahl. About thirty workmen from this locality had been for several months employed in the construction of the new prisons at Wenersborg, above Trollhätta. The first cases of cholera in Wenersborg were reported on the 10th of October; and on the 13th of that month, thirty-three of these men embarked in the steam-boat *Arvika* to return to their homes. On the 15th, they landed at the village of Arvika, and then separated into parties to make their way home. One of these men, Hakan Nilsson, was attacked on the succeeding day, and died at a hamlet on the road. His brother, who lived at a considerable distance, hearing of his illness, hastened to see him, but Hakan Nilsson was dead before he arrived. The brother immediately returned to his own home, and died there on the night of the 19th, of cholera. The clothes of Nilsson were carefully fumigated, and the malady did not spread. A larger party of these labourers took a route to the eastward of Arvika, and crossing a considerable tract of country, rowed over the Fryxen lake to Ostra Emtervik, and there separated to go to their different dwellings. Three of this party were now attacked with cholera—viz., Jöns Olsson on the 17th, Jan Jonsson on the 19th, and Olof Jonsson on the 20th. The two first died, each after 24 hours' illness; the last recovered. All these men lived in separate cottages, at a considerable distance from each other. On the 22nd, Mans Ersson, who inhabited the same room as the above-named Olof Jonsson, was attacked with cholera, and expired after five hours of suffering. This man had not, for a long period, left his home, and had had no communication with any infected locality or person, excepting with Olof Jonsson. The same night, Olof Olsson, dwelling at Södra As, sickened of cholera, but recovered. He had been foreman of the workmen at the prisons at Wenersborg, and had also visited Jöns Olsson when the latter was taken ill. On the 23rd, Jan Nilsson, another of the workmen from Wenersborg, was seized with cholera. On the 24th, Sven Pettersson and his wife, residing at a little distance from the hamlet of Lerbratarne, where the above cases occurred, both sickened of cholera, and these individuals had, a day or two before, visited the people at the above-named hamlet. On the 25th, Brita Olsdotter, widow of Mans Ersson before mentioned, was attacked with cholera, as was likewise Olof Jansson in Ostmanby, whose son had been one of the workmen at Wenersborg, and had returned with them, but was not himself affected. The village of Lerbratarne was now carefully secluded, and no further cases appeared.

We have now examined the details presented to us of the progress of cholera through the south-western part of Sweden in 1850, and shall next briefly call the attention of our readers to the able *resumé* given by Dr. Berg of its ravages, as compared with those of the pestilence of 1834, in the same kingdom. The extreme points reached by the cholera in 1850, extended in Sweden from 55° to 60° north latitude, and from 29° to 34° longitude east from Ferro. Within this area, however, only a few localities were affected, and many of these lay at considerable distances from others. On the eastern side of Sweden, cholera appeared only in one spot—viz., at the port of Döderhultsvik, from which, however, it did not spread into the surrounding country. No conclusions can, we think, be drawn from the geological character of the infected district, though Malmö and the island of Gottland differ materially in this respect from the general gneiss formation of the rest of Sweden.

In 1834 the pestilence advanced on the eastern coast nearly 30 Swedish miles farther to the north, while the western districts, and Götheborg among the rest, suffered severely. From Götheborg, in 1834, the malady spread in a north-east direction as it did in 1850, following the course of traffic along the Götha river up to the Great Wener lake. Besides the capital Stockholm, eighteen other provinces were affected in 1834; but Malmö and Gottland then escaped. Götheborg was ravaged on both occasions; but in 1850 Stockholm remained free from cholera. In 1834 the disease lasted from the 26th of July to the end of December; in 1850 it continued its ravages for nearly five months, or from the beginning of August to the end of December. The greatest spread of the pestilence in 1850 took place during the cold and rough weather which prevailed from the 17th of September to the 17th of October, a period when the diarrhœas and dysenteries incident to the dog-days had almost entirely subsided. In 1834 the mortality from cholera in Sweden was 12,637; in 1850 it amounted to only 1761 deaths. In 1834 more than 12,000 of the above-mentioned deaths occurred in the months of August and September; in 1850, there was a mortality of 422 in these two months, and of 1309 in October, November, and December. Dr. Berg thinks that this great difference in the relative period of mortality in these two years may be accounted for by the fact, that Götheborg, the centre of trade and on the great highway to the rest of Sweden, was one of the first localities affected when cholera appeared in 1834; while in 1850, it lingered long in Malmö, an outlying spot with but little trade.

Of the 4410 cases of cholera in 1850, 2647 were in the towns, and 425 in divers villages and hamlets; while 1338 occurred in the country. Among these last, we have included about 80 cases among the sailors in vessels on the Götha river, where the greatest comparative mortality was observed. In 1834, at least 25,000 cases of cholera were reported in Sweden. It is evident, therefore, that the pestilence of 1850 was much less fatal and less widely spread, the comparative mortality being only 39 per cent. to 50 per cent. in 1834. In 1850, there sickened of cholera 2207 males, of whom 975 died; and of females 1684, of whom there died 745. In 1834, the deaths among females were nearly 100 above those of the other sex. In 1850, the pestilence raged chiefly among the labouring classes, especially among those denominated free labourers (*fria arbetare*), who are not lodged and boarded at the various houses or establishments where they work. Very few of the higher, or even of the middle classes of society, were affected, while in 1834 many of the better ranks fell victims to it.

Dr. Berg next presents us with some observations on the assertion made by the English Board of Health in 1850 in reference to the cholera of 1848-49—viz.,

> "That in every European town where cholera appeared, it gave warning of its approach by the increased prevalence of zymotic diseases, such as influenza, scarlet fever, dysentery, and diarrhœa, and that the last-named, as a premonitory symptom, invariably prevailed extensively before cholera broke out."

On this question, Dr. Berg remarks that agues and small-pox were extremely prevalent in Sweden in 1847 and 1848, that the former of these maladies spread over the whole country at that time to an unprecedented degree, but that in 1849 its limits were much narrowed, and in 1850 only a few isolated cases occurred. The progress of small-pox was not in the slightest degree altered or arrested by the cholera; scarlet fever, of a mild character, prevailed in several districts, but did not increase before the appearance of the pestilence, while gastric nervous fever (typhus) rarely occurred, and never became epidemic. Indeed, the health of the whole country previous to August 1850, was for four years remarkably good. Diarrhœa, dysentery,* and cholerine, appeared annually in summer and in autumn, and were perhaps, indeed, more abundant in 1850 than in the three previous years; but they were not in any way confined to those districts where cholera shortly afterwards broke out. Influenza did not show itself till 1851, when cholera had entirely ceased.

Dr. Berg therefore comes to the conclusion, that

> "It was not possible in 1850 to foretel in Sweden the near approach of cholera from the augmented prevalence or mortality of the so-called zymotic diseases, and that if it be true that cholera is always preceded by an increase of these maladies, and perhaps forms a species of culminating point, or is a product of such culmination of any zymotic disorder, it would be labour in vain to seek how the disease arose, or how it was propagated."

The prevalence of diarrhœa and of dysentery in Sweden during hot weather, has been proved by the carefully kept statistical reports of that kingdom, and which extend back for nearly a century, to be dependent upon atmospheric changes; and the frequency of the above-named disorders during the hot summers of 1834 and 1850 does not appear to be in any way connected with the appearance of cholera in those years. Summers quite as hot, atmospheric vicissitudes quite as great, have repeatedly occurred, and have almost always been followed or accompanied by a great increase of cases of diarrhœa and dysentery, and yet no cholera has been produced by these. Indeed, with the exception of the town of Malmö, where the first cases of cholera were observed during the prevalence of a predisposition to diarrhœa and cholerine, that predisposition, which had manifested itself so generally throughout in the hot summer months, had ceased and almost entirely disappeared for weeks, nay in some instances for months, before cholera broke out. In many places, too, where the pestilence numbered many victims, no diarrhœa, dysentery, or cholerine had been observed during the preceding twelve months; and again these disorders prevailed with great severity in other districts which entirely escaped the ravages of the pestilence.

At Wenersberg, Kongelf, and elsewhere, diarrhœa became frequent about a week before cholera fairly broke out, but this disorder is by Dr. Berg regarded as the result of the pestilence itself. It is true that in most zymotic disorders the first cases in any locality are generally the most severe, but, on the other hand, where the invasion of a terrible pestilence is feared, the earliest cases are too often carefully concealed, especially if the quarantine regulations have been infringed by those who are thus affected. During the prevalence of cholera, diarrhœa was very frequent, and if not checked early, it often ran on into well-marked cholera. In Sweden, as elsewhere, the well-known premonitory diarrhœa was not wanting in individual cases, while in other instances, cholera with all its terrible train of symptoms was developed almost at once.

Dr. Berg approaches the all-important question of the mode in which cholera is propagated, with becoming caution. It is evident that our Swedish colleague cannot divest himself of a certain degree of awe on entering the lists against so imposing a body as that of the English Board of Health. Still the facts elicited from these Reports appear to him (and we own to ourselves also) of so convincing a character, that he does not hesitate, after sundry apologies for his boldness, to place himself in opposition to the opinions expressed in the recent Report of the Board of Health in England, and which has been more than once considered in the pages of this journal. A brief *resumé* of Dr. Berg's opinions on this subject is all that we can here present to our readers; a full translation of this, the most valuable part of the Report, would lead us beyond the limits of the space assigned to us. Dr. Berg has divided this portion of the subject into several distinct heads.

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* Dysentery is, in some provinces, as has been shown by Dr. Huss, almost endemic, and occasionally it becomes extremely severe and epidemic. Thus in the Dales province (Dalarne) dysentery was remarkably prevalent i[illegible]838, but this province has always escaped the ravages of the cholera.

1. The appearance and spread of the pestilence on board ships coming from infected ports.

2. Its influence on the officers &c. at the different quarantine stations on the coast.

3. The mode in which the malady spread along the chief lines of communication throughout the country.

4. The number of *first* cases in previously healthy localities, proved to have been in communication with infected districts or persons.

5. The influence of infected persons on their friends, relatives, and attendants.

To each of these we now invite the brief attention of our readers.

1. *The appearance and spread of the pestilence on board ships coming from infected ports.*

Many of these vessels had been several days at sea before cholera broke out on board, and the malady spread from one to another of the crew and carried off many victims. These ships were then sailing on the Baltic in different directions, and it might be urged that they by chance had encountered a current of air charged with cholera miasm, but it is strange that other vessels, which had not communicated with Lubeck, Stralsund, and other towns where cholera was then raging, did not encounter this supposed poisoned current, or at all events passed through it in safety.

2. Of the physicians, servants, and officers, attached to the different quarantine stations on the coasts, eight sickened of cholera, and two died.

3. *The mode in which the disorder spread along the great lines of traffic.*

When cholera broke out, about the 22nd of September, 1850, in the large town of Götheborg, no signs of the pestilence had appeared in the surrounding villages on any side; but after a longer or a shorter period, the malady radiated from Götheborg as a centre, to almost all those parishes, villages, and hamlets which were in constant communication with the town. It was observed, however, that cholera adhered to what has been so often remarked upon, yet never explained by the anti-contagionists—a pre-eminently erratic course. At a single bound, and before any of the intervening parishes were affected, it passed to Lilla Edet, 25 miles up the Gotha river, the great channel of communication with the interior of Sweden. Here it showed itself first on the 28th of September, in the person of a sailor who had that day arrived from Götheborg, and from this man the disease spread to other persons in the village. Almost all the parishes around Götheborg were, as before said, sooner or later infected, but in the most irregular manner; the pestilence would at one time break out in a parish lying some miles to the north, and then, a week after, a single commune to the southward of the town became, in its turn, infected. In the very next parish, perhaps, no case of cholera occurred till ten days or a fortnight later, and we consider this erratic property of the pestilence as one of the strongest proofs of its being transmitted by individual influence, and of its not partaking of the nature of an atmospheric or telluric miasm.

4. Of the 80 communes or parishes that were visited by cholera in 1850, not less than 50 reported that the first cases of cholera occurring within their boundaries were observed in individuals who had either themselves visited infected places, or who received visits from such localities. The parishes around Götheborg took for the most part few or no precautions, but kept up their daily intercourse with the town, and we accordingly find by the appended map, that scarcely a single parish in that district escaped being sooner or later visited by the disease.

5. The influence of the affected persons on their attendants and relatives has been already several times alluded to, but upon this part of the subject we prefer to give a faithful transcript of Dr. Berg's own opinions:

> "The reports that have been laid before us appear not only to establish the fact that the visit of a healthy person to an infected locality may produce in such an individual an attack of cholera, either immediately or not till some days after he has returned to his yet healthy residence, but also, 1, that in spite of the salutary and purifying influence which the sea air and the sea breezes may exercise upon a ship which has left an infected port, cholera may break out on board of such a vessel at any time during at least fourteen days after she has been at sea.* We must either admit that in such a case the disease has been caught in the infected port, and has lain dormant for a time in the system of its victims, the premonitory diarrhœa, the first symptom often of the malady having been overlooked or concealed, or else that the ship itself has become the receptacle of a store of miasm while sojourning in port, and which pestilential vapour or miasm breaking forth, at length affects the crew with cholera."

It has, however, rarely happened that many of a ship's crew were seized at once; the disorder has crept from individual to individual in a fashion eminently favourable to the doctrine of contagion.

> "2. That after individuals affected with cholera have been removed from such ships into a quarantine hospital, the disease has broken out among the officers and servants of the previously healthy establishment, and it is therefore hardly possible to disbelieve, in such cases, the influence of personal contact in spreading the disease.
>
> "3. That it repeatedly happened, that when a previously healthy individual had had communication with an infected person or locality, and had returned to his own home, where no cholera had hitherto appeared, he has sickened of the pestilence, and that the next victims have been his nearest relatives and friends, and not only those whose condition and habits of life were favourable to the reception of the disease, but even persons living in comfortable circumstances, and that the disease spread again from these new foci. While it is acknowledged that in such cases fear, affliction, and bodily exertion may have a certain influence, yet the effects of personal contact with the sick appear undeniable, unless it can be proved that such depressing influences will of themselves produce an attack of cholera at times when the disease does not prevail in the country.

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* It would be very interesting to obtain some positive data as to the period of *incubation* in cholera as compared with other zymotic diseases. As regards measles, the researches of Dr. Panum in Feroe have, we think, fairly proved that the period of incubation for that disorder is from fourteen to sixteen days.

> "We must therefore come to the conclusion, not only that cholera can be thus propagated from one individual to another, but that this is really the mode by which it is spread over the country.
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> "While we admit that possibly cholera *may* be generated in other countries, and even in Sweden, by local influences alone, the facts elicited in the examination of these reports are, we think, of such force, as to overbalance entirely the negative results obtained as to the origin of the disease in Gotheborg, Ronneby, and Dödörhultsvik, in favour of the origin of cholera in these places from a local cause." (p. 340.)

The fact, that hundreds of persons visited those sick of cholera and yet escaped the disease, is estimated by Dr. Berg at its true value. After remarking that the same is known to occur in all infectious diseases, he admits that something more than mere personal contact, or the inhaling of the miasm from the sick, is required to produce the disorder; that there is some predisposition necessary in the case of the recipient individual, without which he may escape altogether unharmed. What this condition of the system is, we cannot at present define, but we are well convinced that want and misery, uncleanly dwellings and habits, a foul and stagnant atmosphere, and, above all, perhaps, the abuse of spirituous liquors, with its long train of accompanying evils, are causes that most powerfully predispose individuals to receive infection. It is against these, then, that our energies must be directed, in order to mitigate their influence by a well-conducted sanitary reform. We do not mean by this, the boasted activity of well-meaning but mistaken poor-law guardians and magistrates, who, when roused into activity by the near approach of pestilence, enter upon a crusade against the noisome drains, sewers, and privies of our towns; and having perfected their work by whitewashing the walls of each filthy alley, believe that they have done all that is requisite to repel the invading foe;—meanwhile, the lodging-houses, of which the walls have just been cleansed, are permitted to be crowded night and day with the pallid, half-starved victims of intemperance and disease—fit food for any pestilence that may appear; and should cholera be introduced among these, scarcely an individual, as we have ourselves witnessed, will escape the contagion. Without the separation of the sick from the healthy, or at least, unless an abundance of fresh air is permitted to circulate around the beds of the affected persons, cholera will be propagated through contagion in these lodging-houses every time that it visits our shores. Should we, then, have recourse to quarantine regulations for the protection of our coasts? *We think not.* Quarantine, in a country so dependent for its prosperity upon its foreign trade, is, in our opinion, a greater evil than the cholera itself. To be of any avail, such a quarantine involves an almost absolute cessation of intercourse with infected countries; no loophole must be left whereby the disease might creep in amongst us. Is there any one who believes that this is practicable? is there any person who will maintain that by the most stringent penal enactments we can effectually isolate England from the rest of the world? And if we cannot accomplish this, then our imperfect measures of restriction will be infinitely more prejudicial than no quarantine regulations at all. Starvation and misery among the working classes, the inevitable results of the closure of our ports, would create a tenfold predisposition to the disease; and should cholera then be wafted to our shores, twenty victims would fall before it for one that would have died, if the trade of our great commercial country had continued unchecked. In small communities, as in the isolated parishes and thinly scattered villages of Sweden, such prophylactic measures may be, and undoubtedly have been, available, as they have likewise proved efficacious in our northern islands, where, although in a sanitary point of view every circumstance favoured the progress of cholera, yet a strict quarantine enforced on the few vessels that visited their shores, effectually protected these islands from the pestilence. Since, then, we cannot hope to exclude the pestilence altogether, let us oppose every obstacle to its progress, and mitigate the severity of its ravages by effective sanitary measures. On the advantages of sanitary reform all parties are agreed—all acknowledge the necessity of good drainage in our cities, and the protective benefits of cleanliness, temperance, and good food; but the great strongholds of disease—the very foci of infection—our low lodging-houses in the towns, villages, and hamlets, have been too often overlooked; and while deceiving the public by an outward show of cleanliness, their whitewashed walls have concealed a charnel-house within.

Of the care and attention bestowed by Dr. Berg in the preparation of this able Report, it is impossible to speak too highly; and it would form an excellent model to be followed in the documents to be issued by any future Board of Health.

Edward Charlton.

REVIEW III.

What to Observe at the Bedside and after Death in Medical Cases. Published under the authority of the London Medical Society of Observation.—*London*, 1853.

THE little book before us (a convenient pocket volume) enumerates in a very satisfactory manner all the points of importance which are to be taken into consideration in the examination of medical cases, as well at the bedside as after death. It appears that the members of the Society of Observation felt the necessity of performing their examinations, and the arrangement of the symptoms and after-death appearances, according to a certain plan, by which they might be enabled more easily to compare the various results and inferences, and to classify them for statistical purposes. To this end the Society adopted a form which had been framed by Dr. Walshe, and modified by a committee. As it appeared desirable that a fixed form should be used, not only in the limited circle of the Society, but, as much as possible, in the whole profession, the publication of the little volume was resorted to. The Society deserve our best thanks, as their task has been fulfilled with equal zeal and circumspection. Such a work is very useful in many respects, among which the following points deserve particularly to be mentioned:

1. It promotes indirectly the *progress of science.* Though it may contain nothing materially new, yet it is evident that through cases which are elaborated according to the plan laid down in it, science must be more advanced than through others described without a certain plan. Cases given according to a fixed form can be much more easily compared and, for statistical purposes, digested; besides this, the cases elaborated

according to 'What to Observe,' will be as complete as possible; we shall not miss some one or other important point, as so frequently happens in the generality of cases, in which, too often, the very circumstances required for the purpose of drawing inferences are entirely omitted.

2. The work is a very useful one for *practice.* In simple cases the medical practitioner will perhaps not want to look into 'What to Observe,' but he will derive from it great benefit in obscure cases with difficult diagnosis, such as are constantly met with. Frequently it happens, in such circumstances, that even the most experienced and circumspect observer overlooks some one or other point of importance, which does not lie on the surface. Thus it may happen that he completely misunderstands the case. Whoever, under such circumstances, performs a thorough examination, according to this little book, cannot easily neglect any symptom of consequence.

3. It offers much *instruction.* We will not say that it is agreeable and amusing to read 'What to Observe;' on the contrary, its contents are, by the nature of the object, necessarily rather tedious. But in spite of this, we do not hesitate most earnestly to recommend the attentive and *frequently-repeated* study of all its chapters; the younger as well as the elder members of the profession will find it an excellent mode of increasing their medical knowledge. They will not only derive benefit from it for the examination of obscure cases, as already said, but, what is even more important, they will also be always reminded by its study what in the investigation of any disease many things are to be taken into consideration. Every single case, even the apparently most simple one, is more or less frequently very complicated. By constantly being mindful of this, the physician is best protected against the danger of compressing the infinite variety of cases met with in practice into the deficient classes of an unalterable medical system, or of thinking it sufficient to give the diagnosis of a case by one single and necessarily insufficient word, such as "Rheumatism," "Gout," "Pneumonia," "Gastritis."

The arrangement of 'What to Observe' is the following. It is divided into two parts. The first relates to the living patient, the second to the dead body.

Part I. *Clinical examination of a patient.*—Personal description and physiological peculiarities. Previous history. Condition of the patient at the time of observation; generalities; condition of the skin and its appendages; of the organs of locomotion, digestion, respiration; circulation (with the state of the blood); the lymphatic, the uropoetic system (with urine); the organs of generation; the encephalon, with its coverings and appendages; the spinal cord; the organs of the senses; the nerves; the vascular glands. Progress of the case—diet, regimen, external and internal remedies—phenomena of death.

Part II. *Examination of the body after death.*—At first, points to be ascertained and noted prior to commencing an examination. Then, points to be noted *during* an examination—generalities, skin, organs of locomotion, &c., in the same order as just mentioned for the clinical examination. In an *appendix*, particular attention is called to the several objects under the following heads: Condition of the mucous membranes; of the serous membranes; redness; serosity; purulent-looking fluid;

lymph and other exudations: adhesions; abscess; fistulæ; mortification; gangrene-sphacelus; ulcers; perforations; wounds; cicatrices; tumours; cysts; cancer and cancerous-looking matter; tubercles, or tubercular-like bodies; microscopical characters.

The whole account is so complete, that no essential point is wanting, only a few trifling statements might have been added. If we therefore, in the following remarks, mention some one or other subject which we should have annexed, or managed otherwise, we do it not with the purpose of showing a deficiency, but merely with the view that so useful and thankworthy a work may, in a future edition, be free, as much as possible, even of the most unimportant omissions and wants.

To the physiological peculiarities (p. 2) the reviewer thinks desirable to append also a *chemical* history of the individual—i. e., an account of the metamorphosis of matter. As every individual displays certain peculiarities concerning size, weight, &c., so also there are peculiarities in the chemical changes of tissue, in the quantity of the daily excretion of fæces, of urine, of the quantity of solid matter daily contained in the urine, of the various substances composing it—as water, urea, uric acid, salts, &c. The knowledge of these chemical peculiarities is of great importance for the proper understanding of diseases. As, for instance, an individual usually forming or excreting a large quantity of urea, or usually passing through the kidneys a considerable part of the water daily excreted from the body, will bear much worse an affection of the kidneys than another person whose daily excretion of urea is smaller, or in whom a greater proportion of water is excreted through the skin. It cannot be denied that such a chemical exposition of a case is learned with difficulty, therefore those few medical men who recognise the necessity of the investigation draw back during practice, on account of the labour it requires. But as now many of the methods of examination are so much simplified that they may be easily practised, we think that this subject ought not to be omitted in the future edition of a work which treats of the various ways of examining a patient.

Ad No. 115. Physical examination of *the liver.*—It is possible only in few cases accurately to explore the extent of dulness *posteriorly;* but we miss the much more important exploration of the vertical diameter of the dulness in the anterior median line,* which, of course, cannot be performed without examining at the same time for the size and position of the heart.

Ad 184. *Arteries.*—The rigidity of arteries (thickening of the walls with or without deposition of calcareous matter), a point of great importance as the most frequent source of apoplexia sanguinea, appears to be alluded to by "artery tortuous," but we should wish to have it more distinctly mentioned, as it is by the generality of medical men very little noticed, though it must be of considerable significance in the prognosis and treatment.

Ad 220. *Urine.*—In 'What to Observe,' we find it recommended to record the quantity of urine by *weight* (in ounces).[*] This appears to us to be unnecessarily laborious and troublesome. It is much more simple to *measure* the urine and note it by cubic centimetres. We can always do this at the bedside without any loss of time, by making use of graduated urinals. If, at the same time (and it never should be neglected), the specific gravity is taken, the absolute weight may be calculated very easily. Under the head of the *chemical examination* of urine, it might have been mentioned that a quantitative analysis, reduced to 1000 parts, is of very little use, and that this little stands in no proportion to the time employed in it. It is necessary to give always the period during which the urine has been excerned, and to reduce the results, not to 1000 parts, but to one hour or 24 hours.

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* Line from the incisura manubrii sterni to the point of the processus xiphoideus and symphisis pubis. See Chronicle—"Conradi's Paper on the Examination of the Size," &c.

Ad 452, 476.—In the examination of the blood of the dead body, which is best taken from the cavities of the heart, we should wish to draw attention to some points not mentioned. 1. Does the fluid part of the blood, after having been taken out (and placed in a glass), afterwards coagulate or not? 2. Does the blood develope ammonia or sulphuretted hydrogen? To answer this question, a glass stick moistened with hydrochloric acid is held over a glass half filled with the blood (white clouds indicate the development of ammonia). A paper, humected by a solution of acetate of lead, is by means of a cork placed over the blood. Should there be a development of sulphuretted hydrogen, the paper will be blackened after a short time. 3. Under the head of the microscopical examination, we should have taken notice of a method for the approximative estimation of the quantity of the colourless corpuscles (lymph corpuscles) contained in the blood, and of the necessity of an exploration for the accidental admixture of other elements of the body (coagula, caudated cells, cancer cells, &c.).

We should further wish that allusion should be made to the indispensability of a chemical examination of the various organs of the body (under the head of the lungs, the liver, the spleen, the kidneys, &c.) It is true that in most cases it is not easy to fulfil this requirement, but the reviewer has succeeded already now so far in the simplification of the methods for several examinations of that kind, that they ought not to be neglected in any good clinical institution or medical school; and it is to be hoped that this will soon lead to the cultivation of other simple and exact methods. We beg to add a few of the methods which we lately have adopted.

Examination of an organ for the quantity of blood or better of hæmatine contained in it.—We cut the organ (liver, spleen, kidney, &c.) in several pieces, or, if possible, we rub it in pieces, wash it out with water and squeeze it out. The red fluid obtained in this way is collated through a piece of linen, and, as much as possible, freed from solid particles; after this the whole of the fluid is measured. In a carefully measured part of it, we can by means of a scale for the hæmatine,[†] within a few minutes' time calculate the quantity of hæmatine, and through this indirectly that of blood in the organ in question.

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[*] Professor Vogel has been misled here by a slight inaccuracy in 'What to Observe' Strictly speaking, the expression should have been "fluid-ounces," which would have implied measurement. As it is, however, always understood in England, that in speaking of an ounce of fluid we refer to measure and not to weight, the inaccuracy, so far as this country is concerned, is of little consequence. It is very likely, however, to mislead foreigners.—*Editor.*

[†] Archiv des Vereins f. gemeinschaft. Arbeiten. Heft ii. p. 195.

It requires not more time or trouble to ascertain the quantity of *chlorine.* The organ is prepared as just described, but washed out with *distilled* water instead of common water. To a carefully measured quantity of this fluid, nitric acid (free from chlorine) is added in a surplus, and afterwards as much of a nitrate of silver test solution ("*titrirte Loesung,*" i.e., a solution of any test, in this case of nitrate of silver in a certain proportion, by which we know the exact quantity of nitrate of silver contained in a certain measure of the "test solution"), as is sufficient to precipitate all the chlorine contained in the fluid. During the process of adding the test liquid, a part of the fluid mixed with this liquid is filtered; this filtered fluid is examined, partly by nitrate of silver, partly by chloride of sodium, in order to detect whether too little or too much of the test liquid has been added. If the rather dirty part of the rubbing in pieces and washing out can be done by an inferior assistant (a male attendant of the patients, &c.), both the one and the other method will require only a few minutes. Both methods may, of course, as well be used to determine the quantity of hæmatine and chlorine contained in the blood furnished by venesection, &c., and both methods, when properly executed, are sufficiently exact.

After all we have said, we can only highly recommend 'What to Observe,' and most sincerely wish that it may be frequently perused and consulted by every medical man. But we cannot help remarking, at the same time, that we consider the work only as an introduction, as the precursor of a greater, more extensive one, which we trust may soon appear, as we are certainly much in need of it. 'What to Observe' is only an enumeration or a register of *what* we are to observe at the bedside and on the dead body, but which does not profess to mention *how* this is to be done. It is presumed in it, that those who read it are well acquainted with the necessary methods of examination, and are to be reminded only of what they know already. In a few instances only the authors make an exception, as in the method of weighing the liver, heart, lungs, &c., by stating that the gall-bladder is to be attached to the liver, but emptied; that the weight of the lungs is to be taken "after tying the vessels, with trachea and bronchi attached;" that the heart is supposed to be taken out with the venæ cavæ cut through close to the auricle, the aorta and pulmonary arteries just above the free edges of the valves, &c. &c. They are quite right in doing so, as it is impossible to compare the results except the process of weighing has been performed according to the same rules. But the reviewer thinks that an accurate statement of *how* to perform certain processes ought to have been given also in other places. Thus, for instance, how to ascertain the *temperature* (65), which cannot be properly done without paying attention to certain precautions (Traube); how to take the *weight of a body* (66), which ought to be performed either without clothes, or with taking off the weight of the cloth from the entire weight; with an empty stomach, and after the urine and fæces have been emitted. In treating of the physical emanations of the lungs, &c., the authors request that the circumference of the chest be measured during both deep and medium inspiration and expiration; but it would appear to us necessary to add to this, that the circumference is *always* to be measured by taking it during the deepest inspiration and expiration, and putting down the medium of the two; this is the more desirable, as we daily remark mistakes in the description of cases, where the circumference is given without attention being paid to the state of expansion in which the thorax was. We should have further wished that the standard number might have been mentioned in all cases where numbers are in question, in order that those who examine a case might find at once, in consulting the book, whether the number they have found is below or above the average. As, for instance, the average of the weight and size of the liver, the spleen, the kidneys, the heart; the average quantity of urine, &c. &c. It is true that most of these average numbers are yet to be found, and it is to be hoped that the work before us will lead soon to this result, by giving an impulse to such investigations. But for the present, also, an only preliminary communication of such numbers would have been very valuable, and would, no doubt, have led to more frequent examinations, as many practitioners are more inclined for them, if they at once find, by doing so, whether their case, in the one or the other way, deviates from the standard. The reviewer has, for many years, been accustomed to perform such explorations, in order to promote individual medical statistics, and begs to subjoin some of the numbers he has found. He will give only the average numbers, such as may serve in some way as the standard for the practice, adding the outlines of the method by which they are found. Though he has extended his inquiries over the whole of the body, yet he will mention here only those points on which he has the most numerous (in general, several hundred) observations. He begs also to remark, that he gives here only the numbers he has found himself, without comparing them with those of other observers (Reid, Sibson, &c.), as this would lead too far for the present purpose. The weight is expressed in *grammes*, the measure in *centimeters* and cubic centimeters (CCM).

Liver.—Average weight (the gall-bladder being emptied), with the exception of cases of considerable enlargement or shrinking:

Adult men (average of 100 cases) .	1590 grms.	= 56	ozs. *av.*
„ women (average of 40 cases) .	1360 „	48	„

Size of the liver, as found by percussion in the living body. The numbers are the average of several hundred observations of adult male individuals. For women they are not quite so high. They express the whole diameters, from superior border, where it is covered by the lung, downwards to the extreme end of the thin margin (superficial and deepseated dulness together.)

Vertical Diameters:

1. In the *linea axillaris* (from the middle of the axilla to the anterior extremity of the 11th rib)	12 CM.	(4⅔ in.)
2. In the *linea mammalis* (vertical line below the nipple)	12 „	„
3. In the *linea mediana* (the superior margin meets with the heart)	6 „	(2⅓ „)

Horizontal Diameter.

4. The liver extends to the left beyond the linea mediana . . . . 6 CM. ($2\frac{1}{3}$ in.)

(Though these numbers may not be perfectly accurate, yet they are sufficiently correct for the practical purpose, and are easily borne in mind.)

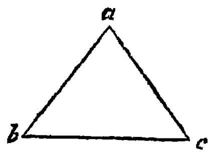
Spleen.—Average weight in adults:

Male (73 cases)	232 grms. =	$8\frac{1}{6}$	ozs. *av.*
Female (25 cases)	181 „	$6\frac{1}{4}$	„

Average diameter of many hundred observations in male living individuals (in the female slightly smaller), and with an empty stomach, as the spleen becomes considerably larger during the act of digestion, some hours after dinner.

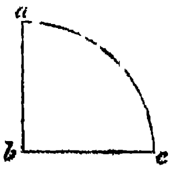
Vertical diameter in the linea axillaris (superficial and deep seated dulness together) . . .	6-8 CM. ($2\frac{1}{3}$-3 in)
Horizontal diameter (the spleen extends beyond the linea axillaris to the right)	3-4 „ ($1\frac{1}{6}$-$1\frac{1}{2}$ „)

Heart.—Average size and position in adult men, as found by percussion:



Deep-seated dulness (*Herzdaempfung*).—In woman, on account of the mammæ, the exploration is often impossible. The following numbers are the average of several hundred cases. The shape represents a triangle with three sides and three corners.

Superior corner (*a*) at the 3rd costal cartilage, or 3rd intercostal space to the left of the sternum. *Right* corner (*b*), in the height of the 5th, more rarely of the 6th costal cartilage, slightly to the right of the right sternal margin, about 3-5 CM. ($1\frac{1}{6}$-$1\frac{1}{4}$ inches) from the linea mediana. *Left* corner (*c*), 5th intercostal space or 6th rib, 10 to 12 CM. (4-$4\frac{2}{3}$ inches) to the left of the linea mediana.—*Length of the sides. Left* side (*ac* = length of the heart) 12-14 CM ($4\frac{2}{3}$-$5\frac{1}{2}$ inches) *Inferior* side (*bc* = breadth of the heart) 13-16 CM. ($5\frac{1}{8}$-$6\frac{1}{3}$ inches.) The exploration of the *right* side (*ab*) has seldom a practical interest.



Superficial dulness (*Herzleerheit*), i.e., that part of the heart which is not covered by the lungs (smaller and disappearing in cases of emphysema pulmonum, larger in cases of hypertrophy of the heart, and of considerable exudation into the pericardium), presents likewise a triangular form. *Superior* corner (*a*), 4-5 left costal cartilage. *Right* corner (*b*), basis of the processus niphoideus. *Left* corner (*c*), 5th intercostal space or 6th rib, 6-8 CM. ($2\frac{1}{3}$-3 inches) from the median line.—*Position and length of sides. Right* side (*ab*), in general corresponding with the right margin of the sternum. *Left* side (*ac*) 6-8 CM. ($2\frac{1}{3}$-3 inches). *Inferior* side (*bc*), 7-9 CM. ($2\frac{4}{5}$-$3\frac{1}{2}$ inches).

Quantitative observations about the URINE.—*In adult men:*

Average *quantity* of 24 hours	1500 to 1600 ccm.	= 52¾ to 56 ozs.
„ „ 1 hour	60 „ 70 „	2⅛ „ 2¾ „
1000 grms. of a man excrete in 1 hour	0·1 „	
100 centim. of height of body are proportionate in 1 hour to about	40 „	

Average specific *gravity*, 1020.

Urea, by the method of *Liebig* (nitrate of mercury):

Average of 24 hours	36 grms.	= 556 grains.
„ 1 hour	1·5 „	= 23·166 „

Chlorine, either by the method of *Liebig* (nitrate of mercury), or by a test solution of *nitrate of silver* (as described above):

Average of 24 hours	10 grms.	= 154·440 grains.
„ 1 hour	0·4 „	= 6·077 „

Free acid (reduced to oxalic acid = O + 3 aq.) by a test solution of *ammonia* or *potass. cdust.*:

Average of 24 hours	2·2 grms.	= 33·970 grains.
„ 1 hour	0·1 „	= 1·5444 „

Phosphoric acid, by *Liebig's* and *Breed's* method, with ferr. chlorid: and soda acet. (test solution):

Average of 24 hours	4·3 grms.	= 66·7 grains.
„ 1 hour	0·18 „	= 2·779 „

Sulphuric acid, by test solution of *chlor. of barium:*

Average of 24 hours	2·0 grms.	= 30·888 grains.
„ 1 hour	0·09 „	= 1·389 „

In taking leave of the volume before us, we must once more express a most sincere wish that the short communications we have ventured to add may lead to further inquiries in this important and interesting field.

J. Vogel (*Giessen*).

REVIEW IV.

Nouvelle Fonction de Foie, considéré comme organe producteur de Matière Sucrée chez l'Homme et les Animaux. Par M. CLAUDE BERNARD.—*Paris*, 1853. pp. 92.

New Function of the Liver, considered as the formative organ of Saccharine Matter in Man and Animals. By CLAUDE BERNARD.

AMONG those who have more recently aspired to advance the science of physiology by experimental inquiry, the name of Claude Bernard is honourably distinguished. He has investigated with patience and ingenuity various problems in the phenomena of life at once difficult and important; and although in some instances his generalizations have been advanced hastily, and in others his experiments have been inconclusive, still, since the days of Magendie, France has produced no physiologist who has more earnestly laboured to elucidate some of the most abstruse functions of the animal economy, or who has accomplished more brilliant discoveries by his researches. To give a succinct account of these discoveries is our object in the present review. We have abstained, except occasionally, from criticism, nor have we thought it necessary to test the validity of everything M. Bernard has advanced. Our chief endeavour has been to place before the reader a clear statement of his labours, and we must leave others to decide on the value of his experiments, and to confirm or refute his inductions. The title of his latest work is placed at the head of the review, but we propose to deal in succession with all his numerous papers, and trust to have omitted from our list none that contain either new or interesting information. Long an assistant of Magendie's in his experimental inquiries, Bernard has derived from that able professor the ready tact by which alone experiments on living animals can be successfully conducted, and the penetration without which it is impossible to interpret aright the results that such inquiries afford. Like most French physiologists, however, he performs vivisections with unscrupulous freedom; and if this method of inquiry has led him to some of his most splendid triumphs, it must be confessed that in many instances he has resorted to it without sufficient necessity, and has thereby exposed a noble science to reproach.

1. *Inquiries into the Nervous System.*—An inquiry into the function of the spinal accessory,* which obtained for its author the prize of experimental physiology from the Academy, is among the earliest of Bernard's productions. A detailed account of the experiments which he performed, and the conclusions to which he was led, while investigating the subject, appeared first in the fourth and fifth volumes of the 'Archives Gén. de Méd,' and has since been separately published in the form of a thesis.† It is, however, unnecessary to enter at length into his views respecting the function of this nerve, because they have been elaborately criticized by able men, and are embodied in all recent text-books on physiology.

Not long after the publication of these researches, Bernard wrote a very interesting paper on the impairment of taste which sometimes accompanies paralysis of the portio dura.‡ He relates four cases in illustration of this singular defection, in each of which the individual affected was unable to distinguish with natural facility the contact of sapid matters, such as quina, or of acids, as the citric. These substances were perceived readily and naturally enough on the sound half of the tongue, but on the affected side they seemed to be tasted with slowness and difficulty, and did not produce the vigorous sapid impression which they impart under healthy conditions to the nerves of taste. The appearance of the tongue was unaltered, its mucous membrane being moist, and as sensitive as ever to the influence of common irritation. The loss of function was always limited to the two anterior thirds of the affected half, and was more or less marked in proportion as the other symptoms of facial palsy attained a greater or less severity. The chorda tympani being the only medium of communication between the facial and gustatory nerves, this impairment of taste was attributed to a cessation of its influence; and Bernard found, while endeavouring to elucidate the subject by experiment, that division of the chorda tympani in the internal ear of animals was followed by a partial loss of taste, similar to that which he had observed in the human subject as a consequence of disease. If, therefore, there is impairment of taste, conjointly with paralysis of one side of the face, it may be reasonably inferred, either that the facial and chorda tympani are both implicated in a common affection, or that the facial is paralyzed above the origin of the chorda tympani. Bernard relates two cases in which cadaveric examination proved the justice of this conclusion. In both there had been facial paralysis, with partial loss of taste in the same side of the tongue, and in both the chorda tympani was involved in the affection. In one of these examples, indeed, the internal ear was the centre of a scrofulous suppuration, which had fairly destroyed the chorda tympani and a considerable portion of the facial.

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* Arch. Gén. de Méd, vols iv. & v. pp. 397 *et seq.*

† Récherches Experimentales sur les fonctions du Nerf Spinal du accessoire de Willis. Par Cl. Bernard. Paris, 1851.

‡ Arch. Gén. de Méd, vol. vi. 1844.

Bernard accounts for this curious phenomenon by supposing that the papillæ are mobile structures, which elongate and contract under the influence of the chorda tympani, and are thus enabled to absorb sapid matters with great rapidity, so that when this nerve is paralyzed, they are no longer capable of adequately discharging their functions.

Against this hypothesis it may be urged, that the distribution of the chorda tympani to the mucous membrane of the tongue, is without anatomical foundation; that the existence of muscular fibres in the papillæ, capable of rapid contraction, and therefore organic, has never been demonstrated, lastly, that it is difficult to understand how the papillary movements, credited by Bernard, can in any way facilitate the perception of impressions by the nerves of taste.

The existence of sensibility in the anterior roots of the spinal nerves, to which Magendie first directed attention, has been since observed by Bernard.* If the spinal cord of a vigorous, well-nourished dog is exposed, it will be found that pain is given when the anterior roots are pinched; and if one of these roots be divided, pain invariably follows irritation of the peripheral extremity, while no effect is produced when it is applied to the central. This sensibility can be no longer excited upon division of the corresponding posterior root. In one remarkable instance, however, Bernard found that it was not completely destroyed, till he had divided the posterior root of the spinal-nerve above. It appears essential to the manifestation of this peculiar sensibility, that the animal should be vigorous and healthy, and that the experiment should be conducted without loss of blood or serious injury. Abstinence from food diminishes it, and long abstinence makes it disappear entirely. Magendie noticed that its existence could not be demonstrated if the animals had lost much blood, or were previously in an anæmic state. It does not, moreover, appear directly after the exposure of the spinal cord, but the animal must be left quiet for a little time, till the immediate effects of the operation have subsided, and the exposed structures have partially regained their natural warmth. These precautions are important; for from inattention thereto, the existence of the recurrent sensibility has been denied by physiologists of high reputation and among others by Longet. Under the administration of ether, the recurrent sensibility departs before that proper to the posterior roots; and it returns later than the sensibility in the posterior roots, and simultaneously with the peripheral or cutaneous sensibility of the body when the anæsthesia is passing away. If, then, the posterior root of a spinal nerve be divided, pain is excited by irritation of the central extremity; and if the anterior root be divided, sensibility is manifested when its distal extremity is irritated, provided no injury has been inflicted on the posterior root, which sensibility is destroyed by division of the posterior root, and disappears when the animal becomes exhausted.

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* Comptes Rendus, 1847, vol. xxv. p. 104.

During the course of last year, Bernard laid before the French Academy* some remarkable observations, resulting from experiments made by him on the sympathetic system. Having divided the sympathetic in the neck of a rabbit, he noticed an elevation of temperature in the tissues on that side of the head, ranging between 5° and 7° Fahrenheit. When contrasted with the uninjured side, this increase of heat was plainly perceptible by the hand, and admitted of accurate measurement by the introduction of the bulb of a thermometer within the nares, or into the external auditory meatus. While, however, this development of heat was most evident on the side of the neck where the sympathetic had been divided, and least so in the opposite corresponding region, where it had been left uninjured, the whole body shared, to a certain extent, in its production, and exhibited evidences of a temperature exceeding the natural standard. Indeed, not much difference was apparent between the warmth of the abdomen and rectum, and that of the side of the head which had undergone mutilation; while, however, the mercury rose to 72° Fah. on the affected side, it was raised only to 68° or 70° by the uninjured one. Nor was this elevation a transitory phenomenon; it persisted with remarkable steadiness till the animals were killed; and even after death, Bernard found the side of the neck on which the experiment had been practised, the last part of the body to lose its vital heat,—the last, in fact, to die. No signs of inflammation or of any other disorder exhibited themselves, to which this augmentation of temperature might be reasonably referred; for although, in the first instance, it was accompanied by increased vigour of the circulation and vascular turgescence, yet these conditions subsided in a few days, while the temperature itself showed no symptoms of diminution.

Some singular results were obtained by exposing rabbits thus mutilated to extremes of heat and cold, the details of which are given in the 'Comptes Rendus.'

2. *Inquiries into the Digestive System.*—We pass from this abstract of the views and experiments of Bernard on the nervous system, to consider his researches respecting the digestion and assimilation of food, the function of the liver, and the organs which participate in sanguification.

One of the most important consequences of these discoveries has been the establishment of the doctrine that animals, like vegetables, are endowed with the power of transforming one ternary principle into another, and also of changing quaternary principles into ternary, by eliminating nitrogen from the former, and converting them into sugar and fat; in short, that the power of chemical combination, as well as of chemical destruction, has been conferred alike on animals and vegetables. Other observers have contributed in no small degree to the recognition of this important truth;

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* Comptes Rendus, vol. xxxiv. p. 471. Premier Semestre, Fevrier, 1852.

but the inquiries instituted by Bernard, with reference to the formation of sugar in the liver, have resulted in proving beyond a doubt that such transformation is a constant and habitual process of the animal economy, and that evidences of its operation may be detected in almost all vertebrate, and in a large number of invertebrate animals.

As Bernard has made out some interesting particulars respecting the secretions of the different salivary glands, a cursory review of this subject will, perhaps, form an appropriate prelude to his experiments on digestion and the formation of blood.

The salivary glands,[*] though analogous in structure, secrete fluids which present dissimilar qualities, and seem destined for dissimilar purposes. The saliva formed by the sublingual gland is tenacious and viscid, and is discharged most freely when mastication is completed, and when the first act of deglutition is on the point of accomplishment, the performance of which it facilitates by covering the arches of the palate and the alimentary mass with a slippery fluid, which enables the food to slide on more easily to the œsophagus. The parotid secretion is limpid, and without viscidity. It flows freely during mastication, and apparently serves the purpose of dissolving some of the more simple constituents of food, and also of softening the whole mass, for the quantity discharged is proportionate to the dryness or humidity of the provision. The fluid formed by the submaxillary gland holds a middle place between the parotid and sublingual secretions, being neither so limpid as the former, nor so tenacious as the latter. Bernard assigns to it the property of communicating sapid impressions to the nerves of taste, on the following grounds. If vinegar is poured down the throat of a mammalian animal in whom the salivary glands have previously been exposed, the submaxillary secretion alone is discharged freely; and if the horizontal portion of the lingual nerve is divided, and the central extremity irritated, a reflex stimulus is the result, which produces a free flow of submaxillary saliva. But the more profuse flow of saliva from the submaxillary than from other glands, when a pungent fluid is placed on the tongue, or irritation applied to the divided gustatory nerve, proves only that the submaxillary is more under the control of reflected or direct nervous influence than the other salivary glands—a circumstance every way compatible with the revelations of anatomy. It proves nothing at all respecting the peculiar power assigned to this secretion of being the exclusive minister to the possession of taste—a property which it is not likely to possess exclusively, and which, at all events, must be vouched for by other and better experiments before its truth can be accepted in physiology. Bernard found that by boiling a portion of either of the salivary glands, the peculiar qualities of the secretion which it furnished were imparted to water. Boiling a portion of the parotid gave a limpid fluid, endowed with all the properties of saliva; and the same process applied to the sublingual gland, produced the viscid secretion which it naturally affords. In this way he was enabled to demonstrate that the labial and buccal glands furnished a tenacious secretion resembling that of the sublingual; and fishes and reptiles, who possess neither cryptiform nor conglomerate salivary glands, have their mouths furnished with a mucous membrane that seems to compensate for the want of those organs, for when boiled it communicates a viscid property to water, like the labial or buccal glands of a mammalian animal.

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[*] Comptes Rendus, Fevrier, 1852, op. cit. Arch. Gén. de Méd., tom. xxviii. 1852, p. 350.

While, however, these secretions exhibit different physical qualities, they are similar in chemical composition, and few substances thrown into the blood escape from that fluid by their intervention. Neither sugar nor salts of iron, nor the ferrocyanide of potassium, could be detected by Bernard in the saliva, after they had been injected into the blood. The sugar which has been found in diabetic sputa is not present in the saliva, but in the bronchial mucus expectorated with it. On the other hand, some salts, such as bromides and iodurets, passed so rapidly into the saliva, as to be able to draw with them salts which, under other circumstances, were not eliminated by this secretion. In this exercise of elective elimination the saliva resembles the gastric juice, urine, and other secretions, each of which fluids is invariably found to contain certain mineral salts, to the exclusion of others. Some later experiments of Bernard seem to show that iodine and its compounds appear in the saliva almost immediately after their entrance into the blood. The same substances show themselves in the bile and urine, after a longer interval; but when all evidence of their presence in these secretions have passed away, they are yet eliminated for some time by the tears, the saliva, and the pancreatic juice. On the other hand, sugar, the ferrocyanides of potassium, and the salts of iron, are excreted by the bile and urine with great rapidity, but never appear in the saliva or pancreatic juice.

The office of the saliva is regarded by Bernard as merely mechanical.[*] It acts, according to him, like so much water moistening the food, and thus facilitating mastication and deglutition. He performed some experiments on horses—which were barbarous, because quite unnecessary—to prove that deglutition is accomplished with far less facility when the supply of saliva is diminished or cut off. It is, however, unnecessary to enter at length into the influence of the saliva on food, because Mialhe and Bouchardat and Sandras have clearly shown that it possesses the power of transforming boiled starch into dextrine, and to a slight extent exercises that power; and Bernard admits that the saliva can effect this change, in common with many other fluids, but says that the food passes into the stomach too rapidly for it to take place under natural circumstances.

Proceeding with the physiology of digestion, and with the investigation of the secretions by which it is accomplished, we are brought to the review of a very able series of papers communicated to the Academy of Sciences by Bernard and Barreswil, in the years 1844 and 45.[†] As, however, most text-books of physiology have *embodied* the substance of these observations in their pages, it will be unnecessary to repeat information already sufficiently known, and we shall therefore confine ourselves to particulars which have been hitherto unnoticed, or not noticed sufficiently, in systematic works. The experiments of Bernard and Barreswil, with reference to the quality of food, and the facility which it presents for assimilation, have furnished results alike interesting and instructive. They were pursued as follows: The substance to be tested was first dissolved in gastric juice collected through a fistula in the stomach, and then injected slowly into the blood of the jugular vein. The urine was subsequently examined, and if the injected material was found therein, it was considered unassimilable; but if the urine manifested no traces of its presence, it was inferred that the injected solution had been appropriated by the blood for the purposes of nutrition. Aqueous solutions of grape-sugar and albumen appeared unchanged in the urine, but when the same substances were dissolved in gastric juice, and then injected, they were assimilated, no traces of their presence in the urine being discovered. Some carefully conducted experiments on dogs seemed to show that sugar and albumen were assimilable, but that gelatine was not.

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[*] Arch. Gén. de Méd., tom. xiii, p. 1.

[†] Comptes Rendus, vol. xviii. p. 783; vol. xix. p. 284; vol. xxi. p. 88. Arch. d'Anatomie, &c., published with the Arch. Gén. de Méd., 1849, volume xi., *Digestion*, par Cl. Bernard.

Bernard and Barreswil repeated these experiments on themselves, and with the same result. Each of them took, when fasting, a quantity of sugar, of albumen, and of gelatine, on separate occasions, and neither could detect sugar or albumen in the urine, but never failed to detect gelatine. As it appeared evident from these results that the gastric juice not only dissolved the constituents of food, but at the same time adapted them for assimilation by the blood; the next object of inquiry was the manner in which this adaptation was accomplished. They were led to believe, from a series of ingenious experiments, that the acidity of the gastric juice was due to the presence of lactic and phosphoric acids: but as subsequent analyses have thrown considerable doubt on the accuracy of this view, it is unnecessary to detail the researches on which it was based. Afterwards they satisfied themselves that there is an organic principle possessing great digestive powers, common to the saliva, gastric, and pancreatic juices, whose mode of action on the constituents of food is determined by the reaction of the medium in which it is placed. If the gastric juice is rendered alkaline, it loses the power of digesting albumen, and acquires that of transforming starch into dextrine and sugar; while the saliva and pancreatic juice, when rendered acid, cease to exert any influence on boiled starch, but act with energy on albumen.

Todd and Bowman, however, assert that they have been unable to find that gastric juice made alkalescent by the addition of an alkaline salt, acquires the faculty of effecting chemical transformations in starch; and probably the whole hypothesis rests on insufficient data. The following experiment of Bernard seems to show that the proper secretion of the stomach takes place only in the pyloric third of that organ. A solution of cyanide of potassium was injected into one of the external jugular veins of a dog, and into the other was thrown a solution of the proto-sulphate of iron. The salts passed out from the blood into the gastric juice, and gave clear evidence of their presence in that secretion by the formation of prussian blue. The colouring of the mucous membrane was, however, confined to the pyloric portion of the stomach, in which spot it made its appearance twenty or thirty minutes before it was observed in any other secretion.*

Blondlot discovered that the albuminous principles of food, if left sufficiently long in contact with the gastric juice, were by that fluid perfectly digested; and if injected into the blood, were assimilated. Bernard repeated the experiments, and confirmed their results.* Both of these physiologists, however, perceived clearly that observations on the changes effected in the food by the gastric juice out of the body, afforded no exact criterion of the office it discharged under natural conditions within the body. It was at once evident that the length of time required for the completion of artificial digestion by the gastric juice alone, offered a strong argument against the belief that any part of the digestive act was completed in the stomach. By subsequent experiments, Bernard convinced himself that the albuminous principles left the stomach changed, but imperfectly digested. The whole alimentary mass passes into the duodenum subdivided and broken up, the soluble and watery parts having been absorbed by the capillaries of the stomach, and the albuminous principles being modified, though as yet unfit for assimilation. In the duodenum and jejunum, the final processes of digestion are accomplished. The bile, pancreatic juice, and the secretions of Brunner's glands, form an intestinal fluid, which acts with energy on all the articles of food, completing the digestion of albuminous materials, transforming starch into glucose and emulsifying fat.

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* American Journal of Medical Sciences, October, 1851.

The experiments of Bernard which relate to the influence exercised on the functions of the stomach by the pneumogastric nerves, are certainly open to objections, and perhaps many of the phenomena observed as consequences of their division might, as Dr. Reid believed, be produced by any severe shock, and would therefore disappear as soon as the effects of that shock had subsided. Bernard, without doubt, killed his animals too quickly after having divided the vagi, for the immediate consequences of so severe an operation to have passed away. Still, making allowance for this, and acknowledging that in some instances the gastric secretion has been restored, that the formation of chyme has recommenced, and nutrition been carried on to all appearance as perfectly as before, yet it seems impossible to deny that an operation, which was followed almost at once† by complete paralysis of the powers of the stomach, which arrested in an instant its peculiar secretion, and converted it, as far as regards the digestion of food, into a lifeless pouch, must have deprived that organ of an influence closely connected with the proper performance of its functions.

The discovery of the changes which oleaginous matters undergo when mingled with the secretion of the pancreas, forms one of the most interesting and important results of Bernard's physiological researches. In an article, the dimensions of which are necessarily limited, it is neither possible nor desirable to detail the experiments and investigations by which the power of emulsifying fat was found to belong especially to the pancreatic juice. Nor, indeed, is such an account needed; for the works of physiology in general use have made the more important of these investigations accessible to every inquirer. But inasmuch as experiments conducted by able hands throw doubts on the entire accuracy of Bernard's conclusions, and unequivocally demonstrate that the inferences he deduced are more sweeping than the results of further inquiry will support, it becomes necessary to glance briefly at the salient features of the question, in order to form a just decision respecting them.

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* Arch. d'Anatomie, op. cit. American Journal of Medical Sciences, op. cit.

† Comptes Rendus, 1844, vol. xviii. p. 995. American Journal, op. cit. Arch. Gén. de Méd., 1844, vol. v. p. 235.

The pancreas, according to Bernard,* secretes a viscid, transparent, and colourless fluid, which exudes during digestion, in large pearly drops, and becomes frothy by agitation. It is formed only during digestion, both Bernard and Frerichs having found the pancreatic duct dry when there was no food in the stomach. Its reaction, according to Magendie and Bernard, is constantly alkaline. On the application of heat it coagulates almost entirely, and is transformed into a white concrete substance. A similar change is effected by the admixture of small quantities of the strong acids, of alcohol, turpentine, and a solution of the metallic salts. Weak acids, such as lactic and acetic, do not coagulate the pancreatic juice. The coagulum thus produced preserves its properties when dried, and communicates them by solution to water. At a temperature of about 100° Fah., oil, butter, or fat, are instantly emulsified, and converted into a white, creamy liquid, resembling chyle, on the addition of a sufficient quantity of pancreatic juice. When placed in a medium, where the thermometer ranges between 40° and 50° Fah., healthy pancreatic juice may be preserved for some days, without manifesting any alteration beyond an increased viscidity; when, however, the temperature of the surrounding medium is raised to 105° or 110°, the secretion in a few hours becomes decomposed, evolving an unpleasant odour, exhibiting a cloudy deposit, and losing the power of coagulating on the application of heat. In the midst of summer this change takes place in a few minutes, so that it is necessary to keep the animal, from which the pancreatic juice is collected, cool, as well as the caoutchouc bladder in which it is received. Division into molecules is not the only modification effected in fat by the instrumentality of the pancreatic juice; for after some hours, the emulsion formed by the mixture of an alkaline secretion with neutral fatty matters, and which is therefore itself alkaline, becomes acid, in consequence of chemical changes, by which the emulsion is converted into glycerine and a fatty acid. This conversion, however, is the result of decomposition, and never takes place, as Frerichs remarks, in the digestive canal, because the other components of the intestinal fluid—such as the bile and gastric juice—interfere to prevent it. It is urged by Dr. Donaldson, that Bernard never maintained that the emulsified fatty matters of food underwent any chemical transformation within the digestive canal, but that he was always disposed to regard this process as the result of decomposition. But whatever may have been the impression on Dr. Donaldson's mind, Bernard clearly seems to believe that the chemical changes which slowly follow the admixture of fatty matters with the pancreatic secretion, are as much dependent on the peculiar influence of that fluid as the emulsification that precedes them. His language is explicit on the point. He states repeatedly that the pancreatic juice first emulsifies the fat, and then converts it into glycerine and a fatty acid; and he nowhere mentions that the latter stages of transformation occur only when the emulsion has been exposed to air, and are the results of a decomposition to which natural digestion offers no similarity. He evidently regards them as changes which the emulsified fat undergoes, as well in natural digestion as in that produced artificially. This, however, is an undoubted error. The experiments of Lenz, Bidder and Schmidt* have placed it beyond question, that the chemical transformation described by Bernard does not naturally occur within the body.

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* Comptes Rendus, 1848, vol. xxviii. p. 259 *et seq.* Arch. Gén. de Méd., vol. xix. p. 60. L'Union Méd., op. cit.

After having fed cats upon butter, they found neither in the small intestines, nor in the chyle, nor yet in the blood, the smallest trace of butyric acid. They next tied the duodenum half-way between the pylorus and the orifice of the bile duct, and immediately afterwards injected into the portion below the ligature some melted butter, which they took care to throw in above the orifice of the biliary and pancreatic duct. After six hours the small intestines contained certainly some butyric acid; and on performing the experiment afresh, having in addition tied the bile duct, the same formation of butyric acid took place. It is therefore evident that decomposition and formation of fatty acids are hindered under the natural conditions of digestion by the gastric juice which is mingled with the chyme.

Bernard recognises two descriptions of pancreatic juice—a normal secretion and a morbid one—the former exhibits the properties and characters previously mentioned; the morbid variety is discharged from the pancreas when that organ has undergone inflammation, however slight, and if the operation for procuring the juice is improperly performed, or prolonged from accidental circumstances, the healthy pancreatic secretion is never obtained. The altered fluid differs considerably from the healthy. It is much less viscid, has a saline and slightly nauseous taste, appears thin and watery, and coagulates feebly and imperfectly on the application of heat or strong acids; it is without action on fat, and rapidly decomposes. The alteration in quality to which the pancreatic secretion is liable, is especially insisted on by Bernard, and should never be overlooked, as it might otherwise become a source of discrepancy and confusion in the subsequent experiments of physiologists. The characters ascribed to the pancreatic juice by Lehmann are certainly those of the morbid secretion, as he himself acknowledges; consequently the assertion of Frerichs that pancreatic juice does not form an emulsion when shaken in a tube with oil, melted butter, or fat, but sinks separately to the bottom, like other fluids, is valueless, because his experiments were performed with the morbid secretion, which Bernard himself states has no action on fat. The manner in which Bernard obtains the pancreatic juice is as follows. An incision is made in the right hypochondrium of a dog, below the free margin of the costal cartilages, so that the operator is enabled to draw out the duodenum, and part of the pancreas. The pancreatic duct is then isolated as quickly as possible, and opened by a fine pair of scissors. A small silver tube is placed in the aperture thus made, and secured in its position by a thread. The duodenum and pancreas are then returned into the abdomen, and the external wound is closed by sutures, care being taken to leave the rim of the silver tube slightly projecting, so that the pancreatic secretion may be discharged externally. After the alkalescence of the first drops of the exuding fluid have been ascertained, a small bladder of caoutchouc is attached to the end of the tube in which the secretion is collected. Bernard consequently obtains the pancreatic juice before it has reached the intestine, and we think his operation preferable to that of Frerichs, being less likely to excite inflammation of the pancreas, and alteration of its secretion.

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* Lehmann: Phys. Chemie, Zweiter Band, p. 110. Lenz: De adipis concoct. et absorptione.

The proceeding adopted by Frerichs, and recommended by Lehmann, is conducted as follows. An incision from two to three inches long is made in the linea alba, and the descending portion of the duodenum is then laid open; a ligature must be placed round the bile duct, before it pierces the intestinal wall, and a small silver canula is then passed into the pancreatic duct from the intestinal tube, through which the secretion of the pancreas may be obtained. Now this operation is open to serious objections. It inflicts more injury than that practised by Bernard; and is therefore more likely to create disturbance. The mucous membrane of the duodenum, no longer lubricated by the bile or pancreatic juice, and irritated by the presence of a foreign body, suffers inflammation, and the pancreas itself falls into a similar state, either from sympathy, or by direct propagation along the duct It is by no means unlikely that gastric juice, or intestinal mucus may find its way down the tube, and disorder both the pancreas and its secretion. Moreover, the fluid obtained by this method must be collected with difficulty, and is likely to be impure. For these reasons the characters assigned by Lehmann to the pancreatic juice must be received with doubt; nor can any importance be attached to the experiments performed by Frerichs with the secretion obtained by his own method, because that secretion was almost certainly either morbid or impure." Frerichs, Bidder, and Schmidt have, however, proved that the pancreatic juice is not the only fluid by which fat is emulsified during digestion. They tied the pancreatic duct in cats, and then deprived the animals of food for twelve or twenty-four hours, so that the intestines might be free from pancreatic juice; they then fed these animals with milk, rich fatty food, or butter, and killed them after six or eight hours. After having repeated such experiments several times, they invariably found the lacteals injected with white chyle, and the receptaculum chyli full of the same fluid. Frerichs cut the intestinal tube of a cat in half, and having injected olive oil, he tied the open extremity of each portion; on killing the animal, the lacteals in the upper half were full of milky chyle, while those in the lower half were much less injected. Frerichs, therefore, concludes that the emulsification of fatty food is mainly effected by the joint action of the bile and pancreatic juice. The experiment with the rabbit, so much relied on by Bernard, and confirmed by Todd and Bowman, is delusive. Bernard having observed that the pancreatic duct in rabbits opened twenty-five centimeters below the orifice of the bile duct, gave to one of these animals a quantity of oleaginous food, and subsequently a meal of carrots, and killed it after six hours. When the intestines were opened, it was at once apparent that white chyle was not present above the orifice of the pancreatic duct, for the lacteals were not at all perceptible above this point, while below it they were distended with a milky injection. This experiment was repeated several times by Bidder and Schmidt, who found that the conclusions based upon it by Bernard were erroneous. For if the rabbit was killed two hours after feeding, the lacteals between the pylorus and orifice of the pancreatic duct were filled with chyle; but if four hours were suffered to elapse before death, the lacteals for some distance below the pylorus were found uninjected; while after six hours, white chyle had disappeared from all the lacteals between the pylorus and pancreatic duct; and after eight hours, had receded twenty or thirty centimeters below that duct itself; clearly showing that the absence of chyle from the lacteals above the orifice of the pancreatic duct, was referable to the time which had elapsed from the reception of food to the death of the animal, and not to the influence of the pancreatic juice. Further experiments are requisite to decide the amount of power possessed by the secretion of the pancreas, and the other intestinal juices separately, to modify or transform fat; at present, the only conclusion to be drawn from the conflicting statements of distinguished physiologists is, that the pancreatic juice takes an important share in emulsifying oleaginous food, but that the bile and intestinal juices are themselves endowed with a similar power—a power that is greatly enhanced by the mixture of these various secretions. But, considering that a large portion of the animal kingdom eat little or no fat, and that the herbivora have, as Valentin remarks, a pancreas larger than the carnivora, it can hardly be supposed that the influence exerted by the pancreatic secretion over fat, is by any means its most important function. This fluid seems rather destined to act throughout the animal kingdom on the unazotized constituents of food, transforming starch into sugar, in the herbivora; assisting in the emulsification of fat in the carnivora; and discharging a combined function in man and other omnivorous animals.

The lacteals, says Bernard, absorb only the oleaginous principles of food which have been previously emulsified. The other constituents of chyle are independent of the digested matters in the intestinal tube, and resemble the components of the lymph in character and formation. Chyle, in short, is looked upon by Bernard as lymph holding in suspension emulsified fat.* The experiments of Brodie, Tiedemann and Gmelin, Bouchardat and Sandras, point to the same conclusion, but it is only necessary in this place to refer to those adduced by Bernard in support of his opinion. Sugar injected in large quantities into the stomachs of dogs, cats, and rabbits, was invariably detected with ease in the portal blood, but never in the chyle. The reason for this is as follows: It is essential for the assimilation of all saccharine principles that the sugar they contain should be transformed into glucose, or diabetic sugar. Cane sugar injected in solution into the jugular vein of a rabbit is eliminated by the kidneys; grape-sugar, on the contrary, remains in the blood, is there assimilated, and transformed into other compounds. Now, the conversion of cane-sugar into glucose and other compounds is, according to Bernard, effected by the liver; and hence the necessity that all saccharine principles should be absorbed by the intestinal bloodvessels, as otherwise they would pass unmodified into the circulation, and be excreted as useless to the economy. A solution of the albumen of eggs injected into the jugular vein of a dog or rabbit, appears after a short time in the urine; but if a similar solution be thrown into a branch of the portal vein, the urine undergoes no change in its composition—proving that an alteration is effected in the albumen during its transit through the liver, adapting it for the nutrition of the tissues. Bernard, therefore, maintains that the albuminous principles of food are absorbed wholly by the bloodvessels. But this supposition is not only unproved, but opposed to many important facts. For, granting that vitelline albumen is incapable of being assimilated before undergoing modification, and granting, also, what is by no means clear, that this modification can be effected entirely by the liver, it still requires to be shown that there are no other organs capable of producing a similar alteration. It is certain that the chyle contains three times as much albumen as the lymph, and equally certain that the imperfect albumen present in the lacteals is developed and matured before it mingles with the blood. If, then, we acknowledge, in the immatured albumen, a power of self-development, or if we consider that the change it undergoes is accomplished by the instrumentality of the lymphatic glands, it is difficult to understand how we can disbelieve that the same substance possesses also a power of self-modification, or that the lymphatic glands are unable to adapt it to the nutrition of the tissues. If the lacteals do not possess the power of absorbing albumen, whence is the large quantity of albumen present in the chyle derived? and if the lacteals are capable of absorbing albumen from the interstices of the tissues, why not also from the digested aliments? Many animals, moreover, eat no fat, and consequently do not form white chyle; but shall we deny to such animals the existence of a lacteal system, because in them that system is precluded from carrying an oleaginous emulsion? Does it not rather appear that the least important constituent of chyle is its molecular base, and that compensation is made for the absence of this constituent from the chyle of herbivorous animals, by an increased activity of the hepatic functions? As regards the conversion of quaternary or other ternary compounds into fat, that it is quite unimportant whether the fat is taken up by bloodvessels, or lacteals, is evident, from the circumstance observed by Bernard, of the portal blood always containing as much fat as the contents of the thoracic duct. Cocks, pigeons, and sparrow hawks, when fed on butter or fat, and killed during active digestion, exhibit no appearance of white chyle in the lacteals, but an abundance of emulsified fat in the portal blood; so that there are really good grounds for believing that the oleaginous constituent of chyle is an element incidental, but not essential, to its composition.

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* Comptes Rendus, vol. xxxi. p. 798. L'Union Médicale, 1850, op. cit.

The discovery of the formation of sugar by the liver constitutes the brightest of Bernard's physiological achievements; and it is impossible to estimate too highly the zeal with which he pursued his researches, or the sagacity he displayed in interpreting their results. Pathological phenomena first drew his attention to the subject. It appeared to him a remarkable circumstance that diabetic patients, while restricted most absolutely to azotized food, should yet continue to pass large quantities of sugar with their urine.* To ascertain whence this sugar was derived, and by what means it was formed, he commenced a series of experiments; and after two years of laborious investigation, in which he was greatly assisted by M. Barreswil, he conclusively demonstrated that in a very large proportion of animals the liver is constantly forming sugar out of the azotized or unazotized substances furnished to it by the portal blood. Some idea may be formed of the extent of Bernard's investigations by the number of animals which he succeeded in convincing himself possessed the property of forming sugar at the liver. They are as follow: among the mammalia generally; in all birds; in a large number of fishes, both osseous and cartilaginous; in the pulmonary gasteropoda and acephalous molluscs. Sugar was also found in the crustacean decapoda; but Bernard is not inclined to ascribe any importance to this circumstance, because in animals so low in the scale, the apparatus of nutrition undergoes considerable modifications. Bernard commenced the inquiry by experimenting on two dogs.

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* Arch. Gén. de Méd 1848, vol. xviii. p. 303.

A bitch was killed seven hours after having fed heartily on mutton and the bones of poultry. Blood collected from the cavities of the heart, and which had stood for an hour and a half, furnished an opaline, milky serum, which, when tested, was found to contain sugar. Not the smallest evidence of the presence of sugar in the intestinal canal could be obtained, nor were there any indications of its presence in the urine. The animal was killed while digesting actively.

In the next experiment, a full-grown and well-conditioned dog was left completely without nourishment for two days, and then put to death. Blood from the cavities of the heart afforded serum containing sugar, while not the smallest trace of such formation was perceptible in the stomach or intestinal canal. It was thus clearly demonstrated that the blood contained sugar independently of the nature of the food, or the changes accomplished in digestion; and the question to be determined was the source from which this sugar was derived. To solve this problem, Bernard undertook a second series of experiments.

A full-grown and healthy dog was killed during active digestion, seven hours after having fed heartily on meat and bones. The abdomen was immediately opened, when the digestive organs were seen to be turgid with blood, and the lacteals filled with white chyle. Blood was collected from the portal vein, near the spot where it receives the splenic, and also from the cavities of the heart: some chyle was obtained from the thoracic duct, the contents of the stomach and small intestines were carefully separated, and these various products were severally tested for sugar. There was none in the chyle; not a trace in the chyme, either from the stomach or intestines; but a large quantity was yielded by the serum of the portal blood, and a less amount by the serum of the blood from the right cardiac cavities.

Second experiment: A full-grown dog was killed, after having been kept entirely without sustenance for three days. On inspecting the contents of the abdomen, the digestive organs were found pale and anæmic, and the stomach and intestines in a contracted state. The lacteals were filled with transparent chyle. The serum of the portal blood betrayed distinct evidences of sugar, which was, however, less abundantly present than in the previous experiment. The blood on the right side of the heart also contained sugar, but the chyle not a trace.

Similar experiments, repeatedly made, invariably confirmed these results; but still it appeared improbable that the walls of the portal vein should possess the power of forming sugar, and if not, from whence was the sugar in the portal blood derived? Bernard, believing that one of the great agents in effecting the portal circulation was the compression exerted by the abdominal walls, thought it not improbable, that when that compression was withdrawn, a reflux of blood from the liver into the portal system took place, whereby substances became mingled with the portal blood, otherwise foreign to its composition. The justness of this conjecture was established conclusively by the following experiment:—A dog, while in full digestion of animal food, was killed by section of the medulla oblongata. The abdomen was immediately opened, and ligatures were placed with all possible speed on the veins emanating from the small intestines, not far from their origin—viz., on the splenic vein, some centimeters distant from the spleen, on the pancreatic veins, and on the portal vein, before its entrance into the liver. From all the channels thus obstructed, blood was collected and carefully tested. The food in the intestinal tube was also submitted to examination. No evidences of the presence of sugar could be detected in the blood taken from the various branches of the portal vein, or in the contents of the digestive canal; but when an aperture was made in the portal vein, on the hepatic side of the ligature, the blood regurgitating from the liver furnished evidences of abundance of sugar. Proofs, moreover, of saccharine formation were yielded by the tissue of the liver itself, while examination of the pancreas, spleen, and mesenteric glands afforded no such testimony. Hence it was concluded, that the sugar found on previous occasions in the portal blood, arrived there in consequence of regurgitation from the hepatic veins of the liver —a circumstance dependent on the sudden withdrawal of the pressure by which the abdominal circulation is in great part effected. In order, therefore, to isolate the sugar as much as possible at the place of its production, it became obviously necessary to tie the portal vein close to the liver, immediately after the division of the abdominal wall; and in all subsequent experiments, this practice was invariably adopted.

The following steps are necessary in order to discover the presence of sugar in the hepatic tissue or the blood.* If, says Bernard, a portion of the fresh liver of an animal be broken up in a mortar, and then boiled a few instants with a small quantity of water, the decoction presents an opaline aspect, and exhibits all the characters of a saccharine solution. The serum of recent blood from the hepatic veins or right cavities of the heart, when tried by appropriate tests, afford as decisive proofs of the presence of sugar as the hepatic tissue itself. The presence of sugar in the fluids or tissue submitted to examination, may be proved by the establishment of fermentation, or by reduction of the oxides of silver or copper; but a combination called the "solution of Barreswil" constitutes an accurate and ready test of its presence in any fluid mixture. This is composed by dissolving four scruples of potash, and the same quantity of crystallized carbonate of soda, five scruples of bitartrate of potash, and three of sulphate of copper, in a pint of distilled water; the whole must be heated to boiling, and then filtered. A few drops added to the strained decoction of a solid tissue, or to a fluid containing grape-sugar, will produce, on the application of a spirit-lamp, the reddish yellow colour which shows the formation of the sub-oxide of copper. Sometimes, however, the presence of organic compounds in animal solutions interferes with the action of the test; it is therefore advisable, when blood or a decoction of hepatic tissue is to be examined, to add first a small quantity of acetate of lead, and then filter the fluid, and afterwards, especially when experimenting on the products of herbivorous animals, to throw down the carbonates in the organic solution by the addition of a small quantity of sulphuric acid, and then, after having again filtered, the test of Barreswil may be applied. The hydrated oxide of copper is reduced, and the sugar is transformed into glucic and paraglucic acids. All the efforts of Bernard to isolate this animal sugar in a crystalline form were unsuccessful. The addition of alcohol, followed by gradual evaporation, yielded only a thick syrup, and never a crystallized residue. Bernard is disposed to ascribe this absence of crystallizable power to the presence of salts, and especially of chloride of sodium.

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* L'Union Médicale, op. cit.

In all similar investigations, it is of the utmost importance that the albuminous principles should be separated from the animal solution by the addition of alcohol, and by subsequent evaporation, because, in consequence of their presence, the sugar undergoes rapid destruction, and quickly disappears.

Although the saccharine compound thus yielded by animal juices exhibits, in many respects, a behaviour identical with glucose, it yet manifests characters peculiar to itself, which seem indispensable to its special office in the economy. Bernard calls it diabetic sugar, to which he has always found it exactly analogous. Magendie proved that a small quantity only of glucose could be injected into the blood without being discharged in the urine; whereas five times as much diabetic sugar was injected into the blood without affecting the urine. Astonished at the discovery he had made, and hesitating as to the interpretation of facts, which seemed likely to subvert the received notions of physiological chemistry, Bernard submitted the results of his experiments to Dumas, and repeated them before him. The formation of sugar at the liver, both during digestion and abstinence, irrespective of animal or vegetable food, was demonstrated as unequivocally as before. Dumas, however, suggested that the liver might be endowed with the power of storing-up in its tissue the products of previous saccharine or starchy food, and surrendering the same to the blood by slow instalments. It seemed to him that an adequate explanation of the presence of sugar might be thus afforded, without supposing it to have originated in the continual transformation of other ternary or quaternary compounds But the result of the following experiment is strongly opposed to that hypothesis. A grown dog was kept without food for eight days, and was then nourished by meat alone for eleven days, at the expiration of which he was killed. Sugar was found to exist abundantly both in the blood of the right ventricle and in the tissue of the liver.

Division of the pneumogastric nerves immediately arrests the formation of sugar in the liver. Diseases which exhaust nervous energy are followed by the same result, for which reason it is never found in the human liver, except after sudden death; and even in the last stages of diabetes, during the exhaustion that precedes death, sugar ceases to appear in the urine. A diabetic patient of Andral's was subject to attacks of diarrhœa, during the prevalence of which the urine ceased to contain sugar. Bernard, however, obtained upwards of five drachms of sugar from the liver of an executed criminal, and forty-seven grains from the liver of an ox. He has also collected a large amount from a diabetic patient, who died somewhat quickly from another disease; (and has detected it in the hepatic tissue of two individuals who perished suddenly, one from a gun-shot wound, and the other from disease of the heart. While pursuing his experiments on the formation of sugar, Bernard discovered, accidentally, that puncture of the floor of the fourth ventricle of the rabbit, with a finely-pointed instrument, was followed in a very few minutes by the appearance of sugar in the urine, and by symptoms of uneasiness and nervous depression. The puncture was made in the middle of the calamus scriptorius, just between the filamentous radicles of the auditory and those of the pneumogastric nerves. Subsequent researches have shown that this is not the only part of the nervous system whose irritation causes an increase of the quantity of sugar in the blood. Puncture of the olivary bodies produces the same effect even more strikingly; and Bernard is able to estimate with great exactitude the degree of diabetes that will ensue, according to the amount of irritation inflicted on the nervous centres. Irritation, however, when roughly made, and involving much lesion of the nervous substance, not only fails to augment the quantity of sugar in the blood, but deprives the liver for a time of the power of forming it. To be successful, the irritation must be made delicately, with a rather finely-pointed instrument. The manner in which it influences the liver is unknown. Bernard was disposed at first to believe that the stimulus being conveyed by the fibres of the pneumogastric nerve, excited the hepatic tissue to a more energetic discharge of its functions; but the discovery that irritation of the olivary bodies was followed by glycosuria, even after secretion of the vagi, forced him to relinquish this view. At present he is rather inclined to regard the sympathetic as the agent of transmission; and Dr. Donaldson states,* that a case of diabetes is on record, in which the sympathetic nerve was observed to be four times as large as natural below the diaphragm. It is, however, impossible to attach much importance to a solitary statement of this nature; and it is not at all likely that such an enlargement of the sympathetic is habitual in diabetes, as it could scarcely have been overlooked in the numerous post-mortems made upon patients who have died of this disease.

The sugar formed at the liver is destroyed, says Bernard, at the lungs, and is not to be found under natural circumstances in the blood of the left ventricle. This statement requires confirmation, as it does not appear to rest on refined analyses. But if the whole of the sugar does not disappear while traversing the pulmonary bloodvessels, that the greatest part of it does so is unquestionable. What then becomes of it? How is it decomposed? Bernard conjectures that after leaving the liver it undergoes a gradual fermentation, and becomes transformed into water and lactic acid, and then into carbonic acid, which is exhaled at the lungs. The source of this fermentation is unknown; Bernard believes it to be an organic principle, though he has hitherto failed to isolate it. Both Bernard and Bouchardat agree that Mialhe certainly erred in maintaining that the alkalinity of the blood was alone sufficient to accomplish the destruction of the sugar; and that in diabetes the sugar is not destroyed because the blood is acid, in consequence of the suppression of the cutaneous transpiration. But the alkaline reaction of the blood, though necessary to the decomposition of the sugar, is not of itself competent to accomplish that change; and the blood is neither acid in diabetes, nor after the suppression of the cutaneous transpiration. The manner in which Bernard disposes of the sugar is not altogether satisfactory, for on considering the subject, we shall find that a large number of animals form sugar by their livers; and it has been ascertained by Bernard, that the most actively-breathing animals, such as birds and mammalia, form it in the greatest abundance; while it exists in a far less proportion in the blood of the reptilia, and that no traces of its presence can be discovered in some fishes*. There is evidence enough, and evidence, too, of a striking character, to prove that the formation of sugar by the liver furnishes one of the conditions necessary to the proper performance of respiration. For example,† if artificial breathing be kept up in a decapitated animal, the production of sugar in the liver goes on; and if the lungs are inflated with air mingled with some irritant vapour, such as chlorine, the sugar appears in the urine, and the animal becomes diabetic after death. Moreover, it seems not a little significant that immediately before the blood is sent to the lungs for oxygenation, it is joined by a compound which, after oxygenation, exists in it no longer. It seems therefore reasonable to believe that the sugar is destroyed at the lungs, in order to minister to the functions of respiration, and to the maintenance of animal heat; and it is probably decomposed into water and lactic acid, the acid passing off in combination with soda, while the water transudes the walls of the pulmonary capillaries, to dissolve the oxygen without, by which means the gases concerned in respiration are placed in conditions favourable to their mutual diffusion. This is, however, a pure hypothesis, and it is obvious that many new researches must be instituted, and many new facts brought to light, before we can hope to elucidate the depths of so difficult a subject.

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* American Journal of Medical Sciences, July 1851, p. 38.

Besides the functions already ascribed to it, the liver forms fat during digestion, and discharges it into the blood.‡ This fat is similar to butter, or the fat of milk; and Bernard thinks that this constituent of milk is in all probability derived principally from the liver, for he found that it was formed much more abundantly by that organ in females than in males. The phenomena of fatty degeneration in muscle, the production of adipocire after death, as noticed by Mr. Paget, and the remarkable experiments of Wagner, go far to prove that nitrogenous tissues may part with their nitrogen, and be transformed into fat; and after having conceded to the liver the power of forming sugar from nitrogenous compounds, it is nothing astonishing that it should possess also a similar power of forming fat. Fat and sugar seem, indeed, in a certain degree, to bear a definite relation to one another; the herbivora forming more of the former, and the carnivora more of the latter. Bernard considers that the hepatic fat differs from that of the chyle, in not being resolved into molecules, in being combined with an azotised substance, and in the long resistance which it offers to the action of ether. It passes through the lungs, being only partially destroyed in their capillaries, is found in the left cavities of the heart, and being carried onwards with the arterial blood, is lost in the capillaries of the systemic circulation. No traces of fat can be discovered in the venous blood during health, except in the space intervening between the liver and the lungs. In some diseases, however, fatty particles have been observed in the venous blood, and Bernard believes that an excessive formation of this constituent at the liver may produce fatty diabetes or chylous urine. Further information on this interesting subject may be obtained from Dr. Donaldson's articles in the American journal, before referred to; and from the writings of Liebig, Lehmann, Combe, and Chambers.

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* Ch. Robin and Verdeil

† See an article by Dr. Max. Vernois (Arch. Gén. de Méd. Troisième Série, vol. i. 1853), analyzed in No. xxiv. p. 551.

‡ L'Union Méd. 1850, op. cit.

Bernard asserts that the blood of the hepatic veins contains more fibrine, and fibrine too of a better quality, than the portal blood, which, according to his belief, is produced by the liver. He seems, however, as Dr. Carpenter remarks, to have overlooked entirely the fibrine furnished by the blood of the hepatic artery; and in opposition to his statements, Lehmann maintains that there is really less fibrine in the hepatic venous blood than in the portal, and that the error of supposing the contrary has arisen from inattention to the augmented number of blood-corpuscles in the blood of the hepatic veins, and from confounding the globuline they furnish with fibrine.

3. *Hepatico-renal circulation.*—It is well known that during digestion materials are received into the blood which profoundly influence its composition, and as some of these, if allowed at once to circulate, might be injurious to life, and others would exist in excess, and become on that account pernicious, the whole of the blood thus altered is received into special channels, which breaking up into a capillary network, submit the returning fluid to the action of the liver, where certain principles are wholly eliminated, and others altered in quality, before it is allowed to pass into the circulation, and minister to the nutrition of the tissues.

The portal circulation is carried on, as we have already seen,* in a great measure, by the pressure of the abdominal muscles; but inasmuch as it presents a capillary system at each extremity, and as both systems are simultaneously called upon to discharge active duties, the intestinal capillaries receiving supplies from the digested aliment, while the liver is exercising its peculiar powers on the blood thus replenished, it is manifest that congestion of the whole system, hepatic engorgement, and stagnation of the portal blood, would repeatedly ensue, unless some provision existed for diverting the blood from the portal vein before its entrance into the liver, and so affording relief to the tributaries of that vessel during the congestion consequent on the reception and assimilation of fresh nutritive materials. Bernard asserts that this provision is supplied by the presence of vessels which establish a direct communication between the portal vein before it enters the liver, and the vena cava ascending to the diaphragm. He has as yet only succeeded in demonstrating the existence of these anastomoses in the horse, in which, he says, they may be easily discovered, both by injection thrown into the portal vein, which distends them, and also by the passage of air from the portal vein into the inferior cava, through these very channels. The blood thus transmitted from the portal vein to the inferior cava, charged with new principles, is disposed of in a singular manner. There exists in that portion of the inferior cava which lies behind and is below the orifices of the hepatic veins, a muscular coat of considerable thickness, the contractions of which cause the cava and renal veins to pulsate during digestion, the pulsations not being synchronous with those of the heart. In a rabbit whose abdomen is opened during active digestion, these movements are readily perceived: and in the horse, dissection of the vena cava inferior places the existence of a muscular tunic beyond dispute. But, in addition to this muscularity, the inferior cava of the horse presents two valves, attached to its wall immediately below the orifices of the renal veins. Now, the consequences of this arrangement are as follow:—During digestion the liver becomes congested, the portal blood regurgitates, and would stagnate but for the existence of channels enabling it to pass into the inferior cava below the orifices of the hepatic veins. The blood thus diverted is not permitted at once to mingle with the general circulation before being submitted to glandular action. The muscular coat of the inferior cava contracts, and greatly diminishes its channel; the impeded blood is thus thrown backward on that ascending from the limbs, but the valves below the orifices of the renal veins prohibit further regurgitation, and it is compelled to flow off right and left by the renal veins to the kidneys, which eliminate from it such materials as are excessive and pernicious; and so the urina cibi is constituted. Meanwhile, the order of the circulation is interrupted by the arrest of the blood ascending from the lower limbs, in consequence of the closure of the valves below the renal veins, but this disturbance is provided for by the existence of the venæ azygos, which receive the impeded blood, and convey it to the superior cava.

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* Arch. Gén de Méd., vol. xxiii. p. 300. L'Union Méd., op. cit.

Such are the views of Bernard respecting the "hepatico-renal circulation."* He adduces three notable experiments to support them. In the first, cyanide of potassium, mixed with carbonate of soda, was introduced into the stomach of a rabbit, and in ten minutes the urine exhibited the characteristic blue, on the addition of a few drops of solution of acetate of iron. At the expiration of half an hour the animal was killed, and blood collected from both jugulars and both renal veins; the serum from the former furnished scarcely a trace of the presence of the salt, while that from the renal vessels contained a large quantity. A strong blue colour was also produced by the application of a solution of iron to the cut surface of the kidney; while a similar proceeding was followed only by a faint tinge when applied to other organs

In the second experiment, a solution of cyanide of potassium, in the proportion of 20 parts of the salt to 100 of water, was thrown into the mesenteric vein of a rabbit. The urine in a few minutes contained a large quantity of the salt, but the animal suffered no inconvenience from its presence in the portal blood. When, however, a solution containing two parts of the salt to 100 of water was thrown into the jugular vein, the animal died in a few minutes, before the slightest trace of the poison could be detected in the urine.

A solution of lactate of iron was injected into the subcutaneous cellular tissue on the back of a rabbit, and a solution of prussiate of potash into the cellular tissue of the thigh of the same animal. In a few minutes a blue colour was manifested at the spot occupied by the solution of iron. But when the prussiate of potash was administered by the stomach, and the solution of lactate of iron placed, as before, under the skin of the back, no blue colour was developed in that situation; and while the urine was found by tests to contain a large quantity of the salt, the serum of the jugular vein manifested but slight traces of its presence.

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* American Journal of Medical Sciences, July, op. cit.

Those experiments prove, according to the author, that the urine formed during digestion is secreted from venous blood; and thus, too, the rapid appearance in that secretion of materials received into the stomach (a phenomenon which astonished Sir Everard Home) may be accounted for. Remembering, moreover, that a large number of animals, such as the reptilia, secrete their urine from venous blood, there is no difficulty in believing that, under certain circumstances, mammalia may form theirs in a similar manner. At the same time, the evidence for the "hepatico-renal circulation" is incomplete, and an extended series of anatomical researches are requisite to demonstrate in various animals the existence of the three conditions essential to its establishment—viz., direct anastomoses between the vena porta and inferior cava; muscularity of the latter below the entrance of the hepatic blood; and valves below the orifices of the renal veins. The absence of one of these conditions is sufficient to render the hepatico-renal circulation impossible.

4. *The elimination of urea.*—In the thirteenth volume of the 'Archives Gén. de Médecine,' 4th series, will be found an interesting communication by Bernard and Barreswil on the elimination of urea from the blood after extirpation of the kidneys.* It appeared to them a singular circumstance that whenever the kidneys of an animal had been extirpated, a period of time, varying from twenty-four to forty-eight hours, always elapsed before the blood yielded on analysis any traces of urea; and it immediately became a question in what way the urea escaped from the system during this interval. To ascertain this point, a series of experiments on dogs was instituted. The kidneys of these animals were first extirpated, and the blood and intestinal contents were subsequently carefully tested. Two of the dogs perished quickly after the mutilation, one from peritonitis, and the other from suffocation. In neither of these could any urea be detected in the blood. The gastric fluids liberated, on the addition of caustic potash, a suffocating ammoniacal odour; the intestinal fluid and bile, when similarly treated, disengaged also large quantities of ammonia. In the other experiments, the animals survived longer, and died, finally, in a state of considerable exhaustion. It was observed, in these instances, that the gastric juice was much increased in quantity, and was secreted without intermission both fasting and during digestion. Large quantities of ammoniacal vapour were liberated from it by the addition of caustic potash. No urea could be detected in the blood till the animal had become weak and exhausted, and the quantity of gastric juice secreted had undergone a considerable diminution in consequence.

Bernard and Barreswil conclude, from these experiments, that after the kidneys of an animal have been extirpated, the urea is eliminated by the secretions of the intestinal tube, and chiefly by the gastric juice, in the form of an ammoniacal salt; and that no urea can be detected in the blood till, from progressive diminution of the vital powers, the intestinal fluids become more and more diminished in quantity, and thus the supplementary channels for the separation of urea are cut off. Retention of urea, they argue, is not a direct result of suppression of urine, but rather a consequence of the loss of vigour which follows it; for if the vital powers remain active, the urea escapes from the blood by other portals, when those provided for its natural elimination are closed. True it is, that in no instance was urea, as such, detected in the intestinal contents; but this circumstance is accounted for by the authors, on the supposition that it is decomposed by the acid of the stomach, and transformed into lactate or phosphate of ammonia, and in proof of this, they state that urea, when introduced into the stomach, is always changed into a salt of ammonia before it can be absorbed.

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* Sur les Voies d'Elimination de l'Urée après l'Extirpation des Reins: Bernard et Barreswil, Avril, 1847, Arch. Gén. de Méd., Quatrième Série, p. 449.

5. *The curara poison*—Before concluding, a passing reference may be permitted to the discoveries of Bernard and Pelouze,* respecting the action of the curara poison, and the manner in which it enters the system. So singular are the facts which have been ascertained respecting this poison, that they have been widely published in medical periodicals, and have even found their way into popular literature, it will be unnecessary, therefore, in this place, to do more than refer to the points which relate especially to its mode of action and absorption into the system.

The curara, according to Humboldt, is the aqueous extract of a plant belonging to the order Logonaceæ, prepared by the natives inhabiting the forests which skirt the high Orinoco, the Rio Negro, and the Amazon. It is affirmed by M. Gondet, that the Indians of Messaya are accustomed (during its composition) to mingle with it some drops of poison obtained from the most venomous serpents. Its toxic action closely resembles that of the poison of the viper, for it may be taken with impunity into the stomach, whereas a speedy death invariably follows its injection into the blood, or its introduction into any of the tissues of the body Under its influence an animal falls suddenly dead without uttering a cry, or scarcely even moving a muscle. If, for example, it be placed beneath the skin of a bird, the animal flies away, and in a few seconds falls dead without a movement or a cry. It has been remarked by Bernard and Pelouze, that death is constantly attended by phenomena that indicate a complete extinction of the functions of the nervous system. A minute after death the nerves are flaccid, like those of an animal which has long expired; no reflex movements can be produced; the blood is always black, and frequently it coagulates with slowness and difficulty, and does not redden on exposure to air.

Yet this deadly poison may be taken with impunity into the stomach or intestines. Mucous surfaces prevent its absorption, and if an endosmometer be made of the gastric mucous membrane of a recently killed animal, so that the epithelial surface look outwards, it will be found that endosmosis will ensue between a saccharine solution within the tube and an aqueous solution of the curara without; but that none of the poison will pass through with the endosmotic current into the interior of the instrument. It must not, however, be supposed that the poison decomposes in the stomach, for it remains perfectly unchanged by admixture with the gastric juice, saliva, bile, or pancreatic fluid; but it cannot pass through the mucous membrane to enter the circulation. The lining membrane of the air-cells alone forms an exception to this rule; through it the poison passes without obstruction, and quickly produces death. But this exception is more apparent than real; for it will be remembered that in the air-vesicles the respiratory mucous membrane has lost its characters, and is reduced to a simple layer of fine and almost structureless tissue, devoid of epithelium. These curious facts with reference to the action of the curara may not be easy of explanation, but they prove that imbibition through an epithelial covering is, to a certain extent, an elective act.

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* Comptes Rendus, Deuxième Semestre, 1850 Récherches sur le curare, Bernard et Pelouze. Arch. Gén. de Méd., 1850, vol. xxiv. p. 800.

With the exception of two papers on 'Organic Combinations,' contributed by Bernard to the Arch Gén de Méd., 6th Vol., 4th series, we have analyzed more or less completely all the important productions of his pen; and we have omitted to notice the articles whose title has been mentioned, because much of their subject matter is a repetition of former statements, and because the experiments related by the author are too few for any satisfactory conclusions to be deduced from their results.

The progress of medicine and surgery is essentially dependent on the sure advancement of physiological science. Experiment alone affords but too often a deceptive and uncertain light for the exposure of error and the establishment of truth, and they who, animated by the recital of Bernard's successes, may aspire to imitate his career, must unite skill in the conduct of experimental research to the industry, penetration, and judgment which have enabled him to achieve discoveries that reflect lustre on his name.

Harvey Ludlow.

REVIEW V.

1. *A Bill intituled 'An Act to amend an Act passed in the Ninth Year of Her Majesty, for the Regulation of the Care and Treatment of Lunatics.'*
2. *A Bill intituled 'An Act to consolidate and amend the Laws for the Provision and Regulation of Lunatic Asylums for Counties and Boroughs, and for the Maintenance and Care of Pauper Lunatics, in England.'*
3. *A Bill intituled 'An Act for the Regulation of Proceedings under Commissions of Lunacy, and the Consolidation and Amendment of the Acts respecting Lunatics and their Estates.'*

(Continued from No 24, p 434.)

SINCE the advancing intelligence or scepticism of mankind repudiated the belief in demoniac possession, and insanity began to be recognised as a disease, it has, until recent times, been considered as a disorder of our intellectual nature alone. One great authority decided that it was right reasoning upon wrong premises; others described it as the result of mistaking the ideas of conception for those of sensation—a species of unbridled imagination; and Erskine pronounced that its essence consisted in delusion.

'This last idea has taken so deep a root in the legal mind, that many of our judges are understood to entertain the strongest aversion to extend the privileges of insanity beyond the range of this delusion test—or say, rather, this delusive test—and to consider that to every man who does not believe that his head is an empty saucepan, that he is the fourth person of the Trinity, or some such nonsense, must be allowed the rights and the responsibilities of sanity. Consequently, when a cross-examining barrister, in obedience to "instructions," girds himself to the task of weakening, by sophistry or ridicule, the effect of some damaging medical evidence, his first question is, "Will you have the goodness to define what you mean by the word delusion?" If medical witnesses were of our opinion, they would never use the term, and refer lawyers inquisitive about it to their Becks' and their Taylors.'

The late Dr. Prichard, whose illustrious name recently adorned the Lunacy Commission, perceived that these narrow definitions did not square with the undoubted facts of experience; that many insane persons had no delusions; that they reasoned well upon right premises, and that in them the essence of the thing was perverted emotion: hence he established the existence of Moral Insanity.*

These opinions, which gained ready assent among those who had daily and hourly opportunities of observing the phenomena of diseased mind, did not find credence among the profound intellects of the legal profession—intellects among whom the reason of the thing is paramount, and the motive generally goes for nothing.

A lawyer practising much in lunacy lately told us that the judges had made up their minds not to endure any more nonsense about moral insanity. If so, perhaps they are right, for in some instances it is undoubtedly a knotty point and a difficult matter to distinguish between a man afflicted with this form of disease and the moral condition of a degraded human being, born and nurtured in the lowest depths of ignorance, infamy, and vice. The public have not yet learned to distinguish between the two; and as an example and terror to evil-doers, one of these hung by the neck at the end of a rope will answer quite as well as the other.

It may happen, at no distant period, that the public, from whom in free countries all authority is derived, and whose convictions slowly but surely modify judicial opinion, will become enlightened and decided in its convictions in this matter, and render necessary an amelioration of the common law thereupon.

Sir James Mackintosh observes—

"An abhorrence of crimes, especially of those which indicate an absence of benevolence as well as regard to justice, is peculiarly strong, because well framed penal laws being the lasting declaration of the moral indignation of many generations of mankind, exceedingly strengthen the same feeling in every individual, *as*

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* It may be doubted whether Dr Prichard did more than find a better name for a condition already well observed and accurately recorded Not to mention Pinel's "manie sans delire," Dr Thomas Arnold, of Leicester, who published in 1782, says: "I call that *impulsive insanity* in which the patient is *impelled* to do or say what is highly imprudent, improper, unreasonable, impertinent, ridiculous, or absurd, without sufficient, or with very slight, or with no apparent, cause" He makes of this impulsive insanity a species with four varieties, and he quotes an admirable delineation of the same from Dr. Monro's remarks on Dr. Battie's Treatise on Madness.

long as they remain in unison with the age and country for which they are destined, and, indeed, whenever the laws do not so much deviate from the habitual feelings *as to produce a struggle between law and sentiment* in which it is hard to say on which side success is the most deplorable."

The contention between law and sentiment thus sketched by the hand of a master. appears at the present time to exist in this country, on the responsibility of those morally or partially insane. The law not only rejects from its estimate of insanity the greater portion of the mental faculties, but refuses to recognise degrees in responsibility arising from this source—so that an offender must either be entirely and intellectually insane, or altogether criminal. The growing intelligence of the people is rapidly teaching them that there are infinite grades of responsibility, and that justice demands there should be corresponding grades of penal treatment. Public opinion is not *en rapport* with the common law on these matters, consequently complaints are heard, on the one side, that whenever the slightest doubt exists of a culprit's perfect sanity, juries cannot be brought to convict; and on the other side, assertions are heard, that the criminal law, on all that relates to lunacy and partial responsibility, is antiquated and barbarous; that whereas in nature every thing and every quality is shaded off into its corresponding opposite, at the bar of assize, as at the bar of God, there are but two classes, the sheep and the goats, and that without the divine omniscience to distinguish between the two.

The existence of moral insanity is for medical witnesses a simple question of fact; they observe it in practice, and they bear testimony to these observations in courts; they cannot there, like the toxicologists, exhibit any process of reduction, therefore with the enunciation of their skilled knowledge their responsibilities end, except so far as the duties of good citizenship will make them endeavour to procure modifications of the law where it is wrong or defective.

In seeking for the explanation of legal opinion on this subject, that of the pleaders is worthy of consideration only because from among them the judges are appointed; any mental bias acquired by long habit cannot be suddenly got rid of, by transferring in the evening of life the person who entertains it from the bar to the bench—if the child is father to the man, the barrister is father to the judge. For the influences of the profession on the mind of the barrister we appeal to Archbishop Whately, who, after speaking of a great Amsterdam corn-merchant who had never seen a field of wheat growing, and who would have been greatly at a loss in the *cultivation* of corn, although he had been in a certain way long *conversant about corn*, proceeds—

> "Nearly similar is the experience of a practised lawyer (supposing him to be nothing more) in a case of legislation, because he has long been conversant about *law*, the unreflecting attach great weight to his judgment; whereas his constant habits of fixing his thoughts on what the law is, and withdrawing it from the irrelevant question of what the law *ought to be*—his careful observance of a multitude of rules (which afford the more scope for the display of his skill, in proportion as they are arbitrary, unreasonable, and unaccountable), with a studied indifference as to (that which is foreign to his business) the convenience or inconvenience of those rules, may be expected to operate unfavourably on his judgment in questions of legislation, and are likely to counterbalance the advantages of his superior knowledge, even on such points as do bear on the question." (Rhetoric, Part 2, chap. 3.)

Again:

"The barrister having to plead *various* causes, is called upon to extenuate to-day what he aggravated yesterday—to attach more and less weight at different times to the same kind of evidence—to impugn and to enforce the same principles, according as the interests of his clients may require. But this very circumstance must evidently have a tendency, which ought to be sedulously guarded against, to alienate the mind from the investigation of truth. Bishop Butler observes and laments, that it is very common for men to have a curiosity to know what is *said*, but no curiosity to know what is *true*. ...... A judge, or any one whose business it is to ascertain truth, is to decide according to the *preponderance* of the reasons; but the pleader's business is merely to set forth as forcibly as he can those on his own side; and if he thinks that the habitual practice of this has no tendency to generate in him, morally, any indifference, or intellectually any incompetency, in respect of the ascertainment of truth—if he consider himself quite safe from any such danger—I should say then that he is in very great danger." (Ibid.)

Briefly to make the obvious application of these generalities to the matter in question. A barrister who may be thoroughly conversant with lunacy as it exists in the decisions of the courts, may remain utterly ignorant of it as it exists in the great field of nature; he may never have seen or have conversed with a single lunatic, except under the restraint of judicial formalities: he may know what all the rules and precedents on the subject are, but for want of comparing legal descriptions with the real entities they describe, he may remain perfectly ignorant of what they ought to be. Add to this the professional "tendency to generate moral indifference and intellectual incompetency in respect of the ascertainment of truth," and we need experience little surprise that the law made by men trained in this school is, on the difficult and intricate subject of insanity, at variance with the requirements of science.

Another important element of the opinions on insanity entertained among lawyers, may be found in the influence which the ethical system of Mr. Bentham exercises on the profession. The biography of an eminent law lord recently dead informs us that he had diligently sat at the feet of the great utilitarian philosopher, and that he regulated his actions and opinions by the precepts there obtained. Perhaps the high places of the law contain few other disciples so devoted and sincere, and may contain many who would earnestly repudiate the jurisprudential doctrines of the sect. Notwithstanding which, the ethics of Bentham exercise an influence over the minds of persons who would repel the imputation of being Benthamites on matters of law procedure and reform. According to these ethics, nothing is absolutely right or wrong, moral or immoral, except as it is, or is not, a matter of utility, of profit or loss; virtue is a sum well proved, vice is a mistake, and conscience, which "makes cowards of us all," is but an equation. It is no wonder, therefore, that men believing in a philosophy which developes all the natural and healthy emotions of the human mind from the operation of the reasoning faculties, should feel themselves restrained from allowing the possibility of the secondary and dependent faculties becoming perverted and diseased, while their origin and cause remained healthy. This would be to acknowledge that the healthy action of the reason could, in the same direction, produce two results—namely, a healthy and also a diseased condition of the emotions, which is absurd. The ethical system of utilitarianism would appear, therefore, imperatively to forbid the supposition that moral insanity can exist without previous intellectual disease. Thus the fundamental obstacle in the legal mind to the admission of a just and comprehensive estimate of insanity, may present itself, not in the shape of half a dozen judicial dicta, more or less inconsistent with well authenticated facts and with each other, but in the philosophical leaven of utilitarianism which imbues the profession, and upon which a true mental pathology can never be engrafted.

It ought not to be difficult to succeed in explaining chemical changes, according to the most recent scientific knowledge, to men whose notions lingered in the regions of phlogiston, because opinions concerning physical phenomena easily give way to sounder opinions properly substantiated. But those which respect the noumena of the world of thought and emotion, retain their hold with the tenacity of opinions founded, not upon the testimony of others, but upon that of the monitorial witness which every man carries within him.

Mr. Bentham himself appears to have recognised this difficulty. If his system of utility were fully carried out, it would apparently demand that questions of this difficult nature should be decided upon the testimony of experiments, but he draws a distinction between physical and moral improbability which eludes this necessity.

> "The degree of distrust produced in the mind of a judge by the improbability of the alleged fact, when that improbability is of the physical kind, as above, will depend upon the confidence he has on his own knowledge respecting the powers and order of nature, so far as the particular fact in question is concerned. If he have any doubt, he will do well to have recourse to scientific evidence—to call in the opinion of such persons as, by their professional situation or reputation, are pointed out to him as being peculiarly well informed in relation to matters of that sort.
>
> "Concerning moral improbability as above described, every man acting in the situation of a judge will naturally consider himself as competent to pronounce. A man on these occasions looks into his own mind, and asks, as if it were of himself, whether it be probable, or possible, that in the circumstances in which the person in question is stated by the evidence as entertaining such and such perceptions, conceptions, intentions, wishes, and the like, it could have happened in such circumstances to himself, to have entertained any such perceptions, conceptions, intentions, wishes, and the like." (Bentham's works, vol. vi. p. 153.)

Now questions relating to insanity being "confined to such facts as have their place in the human mind," would, according to this distinction, be withdrawn from the sphere of scientific evidence, and referred to a comparison with the mental operations of the judge. No doubt such comparisons are involuntarily made by all men, and are the source of many judgments and of more sympathies and antipathies; this mode, however, of arriving at truth, is liable to fearful mistakes, and is ill calculated to subserve the ends of impassive and even-handed justice. But it may be asked, Is the evidence of medical witnesses all that could be desired? is it always lucid and simple, bearing the impress and the authority of skill and experience? above all, do medical experts never disagree, and thus mutually neutralize the force of each other's testimony?

Those conversant with lunacy trials will readily make for themselves the humiliating answer. The occasions for that answer are not far to seek. In the first place, the great mass of practitioners feel themselves, year by year, further removed from opportunities of acquiring an efficient practical knowledge of the subject. The legislature imperatively commands all pauper lunatics to be forwarded without delay to the county asylums; so that a medical man in extensive practice cannot, in the bulk of the population, see more than the commencement of insanity cases, without subjecting himself to heavy penalties. Even when insanity occurs among his wealthier patients, and he does not feel himself under this compulsion, well knowing that the probabilities of cure will be small so long as the patient remains at home, in the midst of the influences which have occasioned the malady, he urgently and disinterestedly counsels removal to an asylum. The improved treatment prevailing in most asylums, and the enlightenment of the public mind on these matters, concur to support his counsels From these circumstances it happens that the generality of medical men in practice see very little of the phenomena and treatment of insanity; so little, indeed, do they see, that when the overt act of some neglected lunatic occurs to place one of them in the witness-box, he may feel as much need to grind up for the occasion from Taylor and other text-books as the barristers themselves. Nor is it surprising that the latter gentlemen should have the advantage at this kind of work, seeing that they are in the constant habit of thus getting-up knowledge sufficient for their purposes at short notice, and upon every variety of subject; and that the practised word-fencer must ever beat the unpractised at word-fencing was recognised by Plato in his dialogue between the sophister and Socrates

This removal of lunacy practice from private medical practice can in nowise be compensated for by a little talk about insanity during the hurried years of medical studentship, or by information derivable from a course of lectures at Hanwell, and a course round the wards of the same institution repeated some half score of times, during which the prevailing sentiment among the pupils may be conveyed in that proverb which consigns the most tardy to the enemy of the human race.

In the annual report presented last year by the medical officers of Bethlem, it is stated—

> "There have been latterly a few pupils in attendance, but their main object, with a few exceptions, appears to be, the obtaining a certificate of competence to superintend a county asylum, or some fleeting object of the day; any steady pursuit of a real knowledge of the nature and treatment of insanity is a rare circumstance, and scarcely to be expected among the multifarious objects which necessarily distract the attention of medical students"

With this pretence of instruction, and with no subsequent practice, it may come to happen that witnesses unconnected with the specialty may seldom have reasons for speaking with authority on lunacy; or if any, they may be sounding rather than sound, like those of the gentleman who undertook to teach the German language from his knowledge of the German flute.

But if modern legislation and medical practice tend in this manner to concentrate the knowledge of insanity in a few experts, who make its nature and phenomena their especial study, do these experts always entertain opinions conceived with that unanimity which exalted knowledge should engender? Are the physicians of Hanwell, Bethlem, St. Luke's, and other like institutions, never to be found ranged in the affrays of law against each other, like the Homeric Olympians, mingling in the vulgar feuds of mortals? Alas! human reason is fallible, and even the bench of bishops do not agree on all subjects. There is, however, extraneous to this fallibility, a cause which leads the opinions of skilful and experienced men to diverge from each other; a cause which enters into active operation when the real balance of probabilities approaches nearly to an equipoise, when the matter in dispute is almost a point—a point, however, which may loom large through the haze of conflicting interests. We allude to the manner in which cases are *got-up* by the attorneys. The depreciating opinion of lawyers is well known, that on nice points, if a dozen medical witnesses can be induced to take one view of the question, thirteen can readily be placed in opposition to them who will take the other view. We admit that this allegation is, to some extent, founded upon the realities of experience; we deny that it is dishonourable to the profession. The same array of conflicting opinions may be marshalled on scientific questions of any description not medical, provided they are intricate, disputed, and of balancing probabilities. Thus it has ever been, and thus it must ever be, so long as questions of this nature continue to exist, and the minds of men present their usual varieties of capacity and bias. If discredit attaches anywhere, it attaches to the form of legal procedure, which adopts such clumsy expedients for the investigation of truth and the administration of justice.

Suppose a case in which the main question at issue is the sanity or insanity of an individual; the first step is the appointment, by chance, interest, or merit, of attorneys—for the prosecution and defence, if the case be a criminal one—for the plaintiff and defendant, if it be a civil action. Suppose, further, that the case is not one in which the facts will necessarily carry conviction to the minds of the judge and jury, but will leave full scope to the ability of the attorneys in procuring favourable scientific evidence, and to the skill of the barristers in extolling or depreciating such evidence as may tell for or against the interests of their clients. How do the attorneys proceed to enlist under their respective banners the opinions of disinterested medical men? Why, they ascertain what men of repute are known to entertain general opinions leaning towards stringent or modified responsibility, towards a restricted or relaxed application of the term insanity, and the like. If these gentlemen do not already rank in the highest grade of the profession, and have therefore more desire to gain credit than fear of losing it, so much the better for the lawyer's purpose. These men they call upon, and place before them the facts and arguments bearing on the case, in a manner as favourable to their clients as it is possible to do without palpable departure from the truth. Medical witnesses thus for the first time see the case through the medium of an attorney's spectacles, which are tinged, but certainly not with a neutral tint. They may commence with a hesitating and qualified assent, but they will be fortunate and highly praiseworthy if they permanently repel and subdue every trace of desire that the opinions to which they assent should prevail. If such desire should creep in, it will make them in a commensurate degree partisans. A man who is able on such occasions to resist all approach to partisanship is more or less than human, for partisanship, like laughing, cooking, and the other peculiarities characteristic of the human race, is a part of his nature.

In courts of law, partisanship is rampant; it is the very essence of the barrister's calling. The probability of its existence in the jury-box is recognised by the provisions for challenging, and by allowing the prejudices of foreigners to be represented there when the prisoner is an alien. Even judges cannot be said to be free from it, although what may be considered as a mere frailty in others may become a high crime in them.

"Naturam furcâ expelles, tamen usque recurret"

They cannot see a prepossessing or a repulsive expression in the countenance of a prisoner, without experiencing on that account a feeling of favour or disfavour towards him. But the expression of such feeling, though not unknown, is highly reprehensible, because it would prejudice the jury and prejudge the prisoner's guilt.

Under the existing system of criminal procedure, the conduct of the prosecution and that of the defence are often equally unscrupulous. Can nothing save our courts from the scandal of a system, which is removed but a few years in time, and a still less degree in spirit, from the practice of rewarding with blood-money witnesses on whose evidence a criminal was hung?

For the purpose of ascertaining truth, it appears to us that the duty of setting forth the argument with impartiality and justice, to "nothing extenuate nor set down aught in malice," is as weighty, as important, and as dignified as that of forming a judgment upon the argument and apportioning the penalty.

Why, therefore, are there not public prosecutors?—officials responsible for the dispassionate discharge of those duties which at present devolve upon attorneys, with whom the administration of justice is secondary in importance to the honour and prestige of success!—officials who would supersede the worn-out machinery of the grand jury, and impart certainty, unity, impartiality, and dignity, to those portions of criminal-law procedure which at present are too often distinguished by the opposite qualities!

With public prosecutors, scientific evidence would still be sought, but would no longer be *got-up* as at present. Scientific opinion would no longer be warped at the outset by partial and interested statements; the honest and dignified pursuit of truth and justice on the side of the prosecution, would enforce the same spirit upon the defence; and on questions of medical or other science, skilled witnesses would be called, not to gain a cause, but to elucidate the truth.

But in civil suits involving the question of insanity, and in inquisitions, the appointment of public prosecutors would not be operative; and in these processes, the *getting-up* of scientific evidence is pursued in a manner still more unscrupulous than in criminal-law procedure; the same agencies are even more actively at work, warping opinion and enlisting evidence.

Let it be granted that the attorneys only do their duty. These difficulties appear inseparable from the practice of admitting opinions as evidence, of permitting men to testify not only to their sensations and knowledge, but also to their convictions and judgments. Such mental operations are not evidential, but judicial; and we are compelled to the belief that persons exercising them are out of place in the witness-box, and that the only efficient remedy is to change their position. This may be effected in one of two ways: either by separating the question of insanity from that of guilt, by leaving the latter to be tried in the ordinary manner, and by impannelling a jury of experts to try the former; or, secondly, by adopting a hint from the practice of the Admiralty court, and calling scientific aid to *assist* the judge. Who can doubt, that in questions of salvage, and collision, and barratry, and others involving the art and science of navigation, skilled opinions could be ranged on either side with as much facility and with as neutralizing an effect as in the more intricate questions of insanity, were such opinions called in to assist the plaintiffs and defendants, and not to assist the court! But this stultification of evidence is avoided by calling in skilful and experienced mariners, the Brethren of the Trinity Corporation, not as partisans to assist the plaintiff or defendant, but as *amici curiæ*, to assist the court with their opinions and judgment.

We are convinced that if a similar procedure were adopted in all lunacy trials and inquisitions, the decisions and awards come to by its means would be more satisfactory to the contending parties and to the public, would better promote the ends of justice, would more effectually sustain the honour of the legal and medical professions, and be found in every respect vastly superior to the present one.

From this digressive excursion into the realms of law, we return to those of psychology. Dr. Prichard, as cautious as he was learned and experienced, satisfied himself with recording the facts he had observed in the occurrence of moral without intellectual insanity: as he twisted no especial theory out of these facts, there is none to examine: had he formed one, it would have resolved itself into a part of the larger question to which we must now direct our attention. Those of our readers who have had opportunities of observation will not require that Dr. Prichard's statements should be verified by examples; those who have been less fortunate must refer to his treatise on insanity. As for the lawyers, we must now reluctantly leave them behind; like tail hounds, they are boggling on the cold scent of the last check, while we have had a smart burst and have come to another; they will never come up again unless they run to our cry.

All medical men of experience now acknowledge the occasional existence of mental disease without disorder of the intellectual faculties. The problem now claiming attention is a more advanced and extensive one: namely, whether, with certain admitted and well marked exceptions, insanity does not invariably commence with and consist in emotional disturbance. The exceptions include those cases which by some writers are designated Symptomatic Insanity, and arise from recognised physical causes: from drunkenness, gout, fever, phrenitis, apoplexy, epilepsy, blows on the head, insolation, parturition, old age, &c. We believe that, except in these cases, convincing arguments can be adduced to prove that *insanity is always in the first instance emotional;* that intellectual disturbance is always secondary; and that Dr. Prichard's cases were apparently exceptional, because in them the secondary part of the disease did not occur, from the unusual force of a conservative tendency in the intellectual faculties. Any hesitation we may feel in adopting this theory arises not from any deficiency of argumentative proof, but because in reconciling so many inconsistencies and in smoothing so many difficulties, it appears to favour the dominion of that idol of the tribe which leads us to expect and to require an amount of uniformity which does not exist in the operations of nature.

M. Guislain has recognised some portion of this theory, though he has missed the whole breadth of its truth. He has unnecessarily limited his view to the operation of the painful emotions, and has therefore persuaded himself that mental aberration is but a state of phrenalgia. He may possibly be right, on the ground that pleasurable emotions must become painful from their intensity, before they can produce so serious a result as mental disease. Being tickled to death is said not to be an easy or agreeable mode of dissolution; and by blunting the nervous sensibilities, chloroform might have saved that luckless wight who

"Died of a rose in aromatic pain."

It is to be regretted that M. Guislain should thus have been diverted from a train of ratiocination directly tending to an enlarged emotional theory, by subtile distinctions between pleasure and pain; distinctions which almost lead him to disbelieve the existence of the former. He expresses his concurrence with that accomplished psychologist, M. Brierre de Boismont, who thus sums up his long experience on the causation of insanity:

> "Eh bien, ce que nous avons vu et entendu depuis trente ans, nous donne la conviction inébranlable que la souffrance morale est le lot de l'humanité Quand la statistique, que nous apprécions à sa juste valeur, nous accablerait de ses chiffres, nous ne pourrions nous empecher de dire. ils souffrent; s'ils le nient, ils trompent sciemment. Le bonheur n'a pas d'enseigne."

We do not ourselves concur in the Rasselas sentiment which estimates the lot of mankind as one of inevitable moral suffering. Undoubtedly the life of man is one of effort, of contest, against evil, or of craven submission to it. But so far as we have learned to appreciate the struggle, it is one which, on the whole, elicits and confers far more of happiness than of misery. We entertain a firm conviction that not only in the world at large, but even in the wards of lunatic asylums, the amount of moral enjoyment vastly preponderates over that of moral misery.

The instances of insanity caused by sudden and great success, or elevation to high fortunes, are too numerous and too well authenticated either to be explained away or denied. We cannot, therefore, subscribe either to the major or the minor of the argument which refers the causation of insanity to moral pain alone.

In addition to the above objections is the impossibility of defining for purposes of scientific exactness, the meaning of pain and pleasure. What is pleasure to one man becomes pain to another, or to the same man at another time. King John tells Constance that she is in love with grief; and in reply to Rosalind's taunt, "They say you are a melancholy fellow," Jaques says, "I am so; I do love it better than laughing."

In Devonshire, invalids are popularly said to *enjoy bad health*. Although this expression may be only a vulgar periphrasis, and not intended to convey the meaning of pleasure derived from bodily disease, it may nevertheless sometimes express a fact. The sensations produced by psora are described by writers of authority as rather agreeable than otherwise; and we have read of patients subject to recurrent mania, who looked forward to the excitement of the paroxysm with lively anticipations of pleasure. Certainly there is no accounting for tastes, and the man who can draw a scientific line between pleasure and pain will have something to bequeath to posterity. It will not, however, be by placarding the words *bonheur* and *malheur* in large capitals, after the manner of M. Guislain, that this achievement will be accomplished, and the *mélange* of good and evil to be found in this mortal life be analyzed for the purposes of psychological inquiry. To import the question of pleasure or pain into an inquiry on the causation of insanity, appears to be a gratuitous complication of a subject sufficiently difficult in itself.

Eschewing, therefore the phrenalgic theory as unnecessarily narrow and involved, by what kind of reasoning may we expect to found our theory of insanity upon the broad basis of emotion in general? The arguments adducible for such a purpose might be arranged in two divisions, according as they belong to ethics proper, or to mental pathology. The former would embrace the whole controversy on utilitarianism, on selfish or unselfish motives to action, and cannot, therefore, be introduced in this place. Being convinced that it is impossible to explain the nature of shame, remorse, justice, moral approbation and disapprobation, by the calculations of advantage and disadvantage, we adopt the unselfish theory. We feel assured that "mankind demands of its heroes some other merit than that of a sagacious merchant;" that civil law is not the measure of innocence and crime, or theologic law that of virtue and vice; that innate principles of duty and of right are implanted in the human soul; and that in these principles, and in the varied play of the emotive faculties, is to be found the true key of human action.

That reason, as reason, can never be a motive to action, is thus succinctly demonstrated by Sir James Mackintosh:

> "An emotion has not necessarily anything in common with a perception but that they are both states of mind. We perceive exactly the same qualities in the taste of coffee when we may dislike it, as afterwards when we come to like it. In other words, the perception remains the same when the sensation of pain is changed into the opposite sensation of pleasure. We can easily imagine a percipient and thinking being without a capacity of receiving pleasure or pain. Such a being might perceive what we do; if we could conceive him to reason, he might reason justly; and if he were to judge at all, there seems no reason why he should not judge truly. But what could induce such a being to *will* or to *act?* It seems evident that his existence could only be a state of passive contemplation. Reason, as reason, can never be a motive to action. It is only when we superadd to such a being, sensibility or the capacity of emotion or sentiment, or (what in corporeal cases is called sensation) of desire and aversion, that we introduce him into the world of action. We then clearly discern, that when the conclusion of a process

of reasoning presents to his mind an object of desire, or the means of obtaining it; a motive of action begins to operate; and reason may then, but not till then, have a powerful though indirect influence on the conduct. Let any argument to dissuade a man from immorality be employed, and the issue of it will always appear to be an appeal to a feeling. You prove that drunkenness will probably ruin health—no position founded on experience is more certain; most persons with whom you reason must be as much convinced of it as you are. But your hope of success depends on the drunkard's fear of ill health; and he may always silence your argument by telling you that he loves wine more than he dreads sickness."

The conclusion is obvious. If our reasoning faculties, when in a sound and healthy condition, cannot conduct to action, still less will they be capable of doing so when they are enfeebled by disease.

The pathological argument rests upon the facts well known to physicians, that the causes of insanity are of a nature producing in the first place emotional changes only, either by the sudden and violent agitation of the passions,

> "When all the heartstrings like wild-horses pull
> The heart asunder,"

or by the long-continued influence of circumstances operating more insidiously upon the mind, and producing an habitual state of abnormal feeling.

A man was never yet either reasoned into insanity, or reasoned out of it. The delusion-test may with propriety be upheld by our judges, highly intellectual as they are, and full of dialectic power; but should the test of insanity ever become a part of statute law, it is inconceivable that the large experience of our houses of parliament will permit them to allow it to rest upon any kind or amount of bad reasoning.

The larger portion of the treatment of insanity consists in what is emphatically denominated moral treatment, in restoring the equipoise of the emotions, in repressing the monster passion which swallows up the rest, and in renewing the activity and vigour of the little passions which have been thus unceremoniously dealt with.

No sane man would attempt to reason away the erroneous opinions of the insane; even lunatics of asylum experience come to recognise the hopeless nature of such a task, and respect the delusions of others, although they may be antagonistic to their own.

In the prodromic period of the disorder the emotions are always perverted, while the reason remains intact. In the period of convalescence, the return of correct judgments is an uncertain and fallacious indication of cure, so long as the emotions remain, even in a slight degree, perverted from their normal condition; but immediately the latter are put straight, the cure may be considered complete. Lastly, and chiefly, with the exceptions above indicated, there is no description of insanity which, if traced to its source, will not be found either to consist in perverted emotion, or to emanate from that origin. Such will insanity ever be found by those who diligently investigate its origin, although by secondary disorders of the intellectual faculties it may and usually does become so transformed and disguised, that its essential nature is subsequently not easy of recognition.

In endeavouring to ascertain the nature of insanity by observing and determining the effects of certain causes, the progress of investigation is impeded, by the difficulty of meeting with cases in which the causes are single and simple. In such an investigation, great difficulty arises from the multiplicity of causes, and in a still greater degree from the intermixture of effects. Again, much perplexity is experienced in the observation of effects; for, as the centre of sensation feels not when it is wounded, so the centre of attention can scarcely attend to itself. Few men are able to appreciate the noumena occurring within themselves, and M. Cousin rightly observes that there are not more Des Cartes in the world than Newtons. Were this not so, the almost incredible blindness which left men for more than fifty-six centuries in ignorance of the circulation of the blood, would be as nothing compared with the self-ignorance which left it to David Hartley to make the noble discovery of the simple law under which our passions and affections are formed. When we endeavour to observe the effects of moral agents, not in ourselves but in others, the cloud is still darker. What doth it profit to the end of this inquiry, if, according to the author of 'The Vestiges,' "man in the mass is a mathematical problem," if, "when seen in the individual, he continues to be an enigma"? Suppose it known that one of every six hundred and fifty Frenchmen will commit a crime once a year. This knowledge in itself will not advance us one step towards ascertaining or removing the motives of crime in the enigmatical unit. Keble truly says—

> "Not e'en the tenderest heart, and next our own,
> Knows half the reasons why we smile or sigh.
> Each in his hidden sphere of joy or woe
> Our hermit spirits dwell."

It is true that a madman may often be what Horace says of a drunkard, "pellucidior vitro;" but this species of insane candour is far from being trustworthy, and the difficulty of accurately ascertaining motives, and of observing the operation of pathematic agencies, remains one of the most serious perplexities of the practical psychologist.

In the endeavour to ascertain the antecedents of an attack of insanity, we are particularly liable to be misled by the interested statements of friends and relations, who disguise or deny circumstances which might be thought discreditable to the patient or to themselves. Even when a true history of the case is obtained, its aspect may be very fallacious, as the following well-contrasted examples may serve to illustrate. A seaman returning from the coast with prize money was hocussed, robbed, and immediately became insane. A young Protestant female, on a visit to a Catholic family, came under the influence of the priest, and became a convert: much unpleasant discussion took place in consequence with the members of her own family, and in little more than a year she became insane. In the first case, there appeared to exist the obvious physical cause of poisoned blood; in the second, a no less obvious moral cause. Nothing, however, could be further from the truth, as subsequent knowledge of these cases proved. In the first, the cause was moral, arising from grief; in the second, the cause was physical, arising from semi-starvation and watching during Lent.

For aught we know to the contrary, injuries to the head, poisoned blood, and other similar causes, are capable of producing insanity in any human being; but moral causes, on the contrary, appear to be efficient only when the mind is in a condition favourable to the reception of the morbid influence. When this predisposing condition exists, trivial causes become influential and effective; when it does not exist, men have for lengthened periods been subjected to every kind, degree, and combination of mental suffering, without lapsing from the healthy states of vigorous resistance or of patient endurance. The nature of this predisposing condition appears to consist in abnormal motivity or impressibility of some emotion or set of emotions, combined with a weak and deficient power of will. Too much weight cannot be attached to the recognition of this state of mind, with the view either to the prevention of insanity by a properly adapted education, or with a view to its prodromic treatment.

In investigations concerning mind, it must never be forgotten that a cause, however simple in its nature, does not produce simple and unchanging effects. Geology shows us that the rain-drops of a passing shower may leave their impression upon the soft sand, to remain unchanging and unchanged through countless ages. A like natural phenomenon has often decided the issue of battles, the fate of kings and of nations, has prevented famines, has influenced and is influencing the destiny of the human race. It is said that a rainy day will spoil the best or the worst Parisian revolution. The former example of causation finds no type in the world of mind; the latter is continually represented there. How far did the petty theft of a piece of ribbon influence the life and opinions of Rousseau, and through them the opinions and destiny of the French nation? In mental dynamics, a cause not only produces a train of effects incalculable, from the ever-varying nature of the existence acted upon, but even when simple the cause is oftentimes one of an accumulating sort: like that illustrated by Mr. Mill in the increasing heat of the summer as the days become longer, and the nights during which the heat can be radiated back become shorter, so that the heat accumulates and becomes greater after the solstice than at it—the funded heat of August, as it is called by De Quincy. This description of cause is frequently observed operating in the production of insanity, in unchecked desires and emotions habitually excessive, which, gaining strength with indulgence, eventually overcome all antagonistic and balancing faculties. Of this kind are many causes not strictly moral, but acting upon the emotions; as poverty causing mental distress and incapacity to struggle against it by industry, and consequently greater poverty.

The ever-changing, increasing, and indefinite operation of causes producing insanity, is a strong reason for supposing that their power is exerted upon the emotional part of man's nature. Whatever operates upon the reason produces a definite effect. The intellect cannot refuse either assent or dissent, qualified or entire, to any and every argument or theorem propounded to it. The dialectic function is impassive as machinery, and there is little doubt, if it were possible to submit the premises, that logical engines could be constructed as efficient in their work as Mr. Babbage's celebrated calculating machine, or Mr. Clarke's ingenious instrument for grinding Latin hexameters. The logical engine which every man possesses often does its work badly enough, but whatever the results may be, they are definite and precise.

> "Rage seized him as these contrasting pictures rose before his view. He walked to and fro in disorder, striving to re-collect his thoughts, and reduce himself from the passions of the human heart into *the mere mechanism of calculating intellect.*"*

This intellectual faculty forms an opinion general or particular—erroneous or correct—there it stops; and according to the materials submitted, the quantity and quality of the work done, a man becomes full or empty, stupid or wise, a dolt or a Newton. A man may be an idiot from congenital deficiency of this faculty, or may become demented from decay of it; he may be crotchety from its imperfection, but it appears to us impossible that any condition of the reasoning faculty can produce madness. A medical witness was asked in a lunacy trial whether, if a man believed that a person could see through a three-foot brick wall, such a belief would constitute a delusion, i. e., a criterion of insanity. The unfortunate witness having replied in the affirmative, was of course trotted through an amusing array of consequences, terminating in the melancholy and distressing insanity of all believers in mesmerism. Now, whatever may be thought of the powers of observation and ratiocination displayed by these gentlemen, it must be admitted, that to call them insane is most erroneous; because, with the exception of getting angry when one presumes to argue with them, their opinions do not touch their motives, and consequently do not lead to action Like the necessitarians, they act in opposition to their opinions. The discovery of clairvoyance has not stopped the printing of one newspaper, or lightened one mail-bag; the electric telegraph is as much used as if the stupendous discovery of the snail telegraph had never been made, and notwithstanding the newly perceived translucency of three-foot brick walls, we doubt whether the fact has caused any alteration in the thickness of party walls, or any other architectural arrangements of the most enthusiastic disciple. When a mesmerist judge seriously endeavours to supersede witnesses, counsellors, and jury, by employing sensitive clairvoyants to discover the truth, we think it not altogether improbable that, out of court at least, such proceedings will be considered rather mad.

In 1787, a Dr. Elliot was considered insane, because he anticipated some of the scientific opinions of Sir W. Herschel. He was tried at the Old Bailey for firing a pistol with intent, &c., at a Miss Boydell, thereby burning her clothing and contusing her shoulder. The jury could not find that there was a ball in the pistol, and on this acquitted him. Sir David Brewster, in a note to 'Ferguson's Astronomy,' says:

> "The friends of the Doctor maintained that he was insane, and called several witnesses to establish this point. Among these was Dr. Simmons, who declared that Dr. Elliot had, for some months before, shown a fondness for *the most extravagant opinions*, and in particular, he had sent to him a letter on the light of the celestial bodies, to be communicated to the Royal Society. This letter confirmed Dr. Simmons in the belief that this unhappy man was under the influence of this mental derangement; and as a proof of the correctness of this opinion, he directed the attention of the court to a passage of the letter, in which Dr. Elliot states 'that the light of the sun proceeds from a dense and universal aurora, which may afford ample light to the inhabitants of the sun beneath, and yet be at such a distance aloft as not to annoy them. No objection,' says he, 'ariseth to this great luminary being inhabited; vegetation may obtain there as well as with us. There may be water and dry land, hills and dales, rain and fair weather; and as the

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* My Novel. Bulwer.

light, so the season must be eternal, consequently it may easily be perceived to be by far the most blissful habitation of the whole system.' "*

Our good friends the Mesmerists may, if they think proper, make use of this illustration, and enjoy the fond anticipation that before 1887, a greater name than Elliot may have justice done to it, and that opinions which are now sneered at as illustrative of delusion, will come to be considered as inspirations of the highest genius.

But suppose Dr. Elliot's opinion, instead of a scientific, had been a really foolish one; that instead of a solar, it had taken a lunar direction, and he had thought the moon was made of green cheese, or that, like the wise man of Gotham, he could fish it out of a pond with a net. It does not appear that even such opinions could have induced him to fire a pistol at Miss Boydell, to burn her petticoats and bruise her shoulder: the intellectual mistake could have produced no desire leading to such an act—in other words, the overt act could not have been traced to or connected with the delusion.

Let us now proceed to examine cases in which the insane conduct may be thought to be obviously traceable to and dependent upon delusive opinions.

It was argued that Miss Nottidge was insane, because she believed that Mr. Prince was God; but in Germany a considerable sect of thinkers believed that God exists in every man, and that every man is God as far as he goes; and Spinoza believed that God was matter: therefore, if opinion constitutes madness, and Miss Nottidge was, on that account, insane, *à fortiori*, all these metaphysicians must have been so.

But the opinions of the latter touched not upon motive and action—they scarcely influenced the conduct of life; at most they might possibly tend to keep their advocates out of churches, but could certainly never lead them into asylums. On the other hand, the opinions of the lady probably originated in, and were certainly most intimately connected with, the fondest and deepest emotions of the heart. For them she sacrificed the ties of maternal and sisterly affection, the opinion of the world, and all that she possessed; she left all to follow him. Perhaps her opinions were not more irrational than those of the philosophers, but her emotions were deeply implicated: she entertained towards the creature those *sentiments* which are due only to the awful majesty of the Creator. In such cases, the true test of insanity must be sought for, not in deluded opinion, but in perverted emotion. They supply, therefore, an additional argument in favour of the emotional theory.

Having adduced in support of this theory arguments at least sufficient to establish its claim to a fair and deliberate investigation, we leave it to be tested by time and deductive inquiry. We ask not, and would decline to accept for it, more than an impartial scrutiny, believing that too ready acquiescence in the truth of propositions of this nature is not less inimical to the advancement of knowledge, than dogmatic assertions unsupported by argumentative proof.

Before these pages are presented to the public, the Lunacy Bills which have suggested them will probably have become the law of the land. Their principal, and almost their sole merit, consists in condensation and simplification of the existing statutes. Their demerits of omission are considerable. Many important and pressing questions, such as the treatment of criminal lunatics, are entirely passed over. The visitation of asylums, private and public, is to be left on its present footing—although, as regards the former, the noble chairman of the Commissioners in Lunacy has expressed his opinion in the Lords, that nothing could be more defective and unsatisfactory; and as regards the latter, the utmost diversity of practice exists in different counties—a diversity of practice so contrasted, that the existence of error at one or both extremes is unavoidably impressed upon the conviction.

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* American Journal of Insanity, July, 1852; taken from Notes and Queries.

In some asylums the actual work of visitation has lapsed from the main body of the visitors, and has become the privilege and the glory of some two or three members, who, from a natural and well-founded anxiety concerning the treatment of lunatics, constitute themselves into an hebdomadal board, and become what their own capabilities or those of some adroit matron may be able to develop Surely some reasonable medium might be devised and enforced by enactment, which might rescue the important duty of asylum visitation from degenerating into a formal sham, or developing with exuberant vitality into a mischievous source of excitement to the patients.

Another subject on which the greatest diversity and laxity of practice exists in different county asylums, is the manner of apportioning the expenditure between the maintenance fund and the county rate In several counties a building and repair fund has been established, from moneys strictly and legally, perhaps, belonging to the maintenance fund. the county rates have thus been altogether relieved from the repair charges to which they are liable under the 26th section of the present act. At other asylums all expenditure for the repair of the building, for painting, glazing, and furnishing, is charged on the county rate. In some instances, where the main efforts of the visitors and the superintendents appear directed to the acquisition of credit for extreme economy of management, the most extraordinary items of expenditure are thus charged. Charges for bedding, clothing, attendants' salaries, under the title of artisans' wages, are thus transferred from the poor rate to the county rate. In one county even the chaplain's salary is thus classed with building repairs, and in another the county rate is saddled with the maintenance of the steward-superintendent's pony, though no efforts are made to prevent the medical officer from becoming thoroughly *footsore* Thus, by cooking the accounts, the maintenance charge is kept at a figure astonishingly low. So great is the diversity which exists in the manner of keeping these public accounts, that no fair comparison can be made between the maintenance charges of one asylum and those of another, without an elaborate analysis and rearrangement of all the items of expenditure.

An amended Lunatic Asylum Act might reasonably have been expected to rectify these irregularities yet so far is this from being the case, that in the new bill not only is no attempt made to do so, but even that strange ambiguity of expression is allowed to remain, "shall be claimed to be *leviable, &c.*" (*sec.* 70), which leaves Visitors at liberty to puzzle out, if they can, whether the rent-charge on patients from non-contributing boroughs should be carried to the maintenance fund, or to the relief of the county rate. Even the visitors of two county asylums in the same county have adopted opposite opinions on this point; one board having decided to send this considerable sum to the county treasurer, and the other having passed it to the maintenance account.

Having shown that no attempt has been made to place the question of lunacy on a more philosophical basis, and that even on the important subjects of asylum visitation and expenditure, the most glaring practical defects of the present system are left without amendment, we have no inducement to extend our criticisms to details. We look on the Bills somewhat as a Portuguese fundholder might look upon a new arrangement of the interesting debt of that country—as an advance, namely, towards simplification of arrangement, but as holding forth little prospect of any important results of a beneficial nature.

That they indicate no greater approximation than has hitherto been made between the Law and the Theory of Insanity, can perhaps scarcely with justice be urged as an objection against them, inasmuch as their operation lies out of the pathway of those circumstances which render most apparent the antagonism of true mental science with the lunacy practice in law.

During a recent visit which we paid to that "Cave of Despair," the criminal ward at the Royal Hospital of Bethlem, we were informed that the Government had strictly prohibited any alteration or reform therein. We received this announcement with much satisfaction, as an indication that this department of lunacy treatment was likely to be put on an entirely new footing; that Government believed the affair to be utterly bad, and beyond amendment, and contemplated a revolution therein, in preference to reform.

When new arrangements for criminal lunatics are attempted, if they are not dictated by true psychopathic science, manifest failure will scarcely be avoided. But if in such new arrangements the Law and Theory of Insanity are made to submit to a satisfactory amalgamation, such arrangements may form a *point d'appui* from whence other important reforms of a similar nature will be possible.

That the antiquated barbarisms of our common-law dogmata on the subject of insanity can much longer possess the slightest weight of authority, we do not believe. When the members of our own profession are giving to the world such works as Sir Henry Holland's 'Chapters on Mental Physiology,' and Professor Carpenter's remarkable section, 'On the Functions of the Nervous System'—a chapter which we do not hesitate to characterize as the most profound treatise on the subject which the world has seen—it will be impossible for any length of time to retain lunacy laws founded even a century ago, when physiology was in its infancy; to say nothing of those which took their origin at a time when the nature of insanity was a theological question, and its treatment was confined to the exorcist and the priest.

John Charles Bucknill.

REVIEW VI.

Traité de Chimie Anatomique et Physiologique Normale et Pathologique, &c. Par CHARLES ROBIN, M.D., et F. VERDEIL, M.D., &c.—*Paris,* 1853. Tomes I., II., & III.

Treatise of Anatomical and Physiological Chemistry, Normal and Pathological; or, of the Immediate Principles, normal or morbid, which constitute the body of Man and of the Mammiferæ, &c. By CH. ROBIN and F. VERDEIL.

WE have endeavoured, in a former number of this journal, to give an account, extracted from Messrs. Robin and Verdeil's elaborate treatise, of the 'Immediate Principles which constitute Healthy Human Urine;' we shall now proceed to consider the contents of this work in a more general point of view, insisting upon a few topics, which, from their novelty, or from their classification, deserve particular notice.

Messrs. Robin and Verdeil's book was written with the intention of giving to physicians and anatomists a complete account of the intimate or molecular structure of organic substances in their three fundamental states, liquid, semi-solid, and solid; or, in other words, the authors have endeavoured to describe the various substances or immediate principles which, by their molecular aggregation, constitute organic substances. They do not therefore treat of the organized matter itself, but of its constituents.

Their work is divided into three volumes. The 1st is entirely devoted to general considerations of the 'Immediate Principles;' the 2nd and the 3rd present a systematic view of these substances.

The general feature of this treatise is the systematic classification of the subject. It was impossible that in an undertaking of such magnitude, the authors should have overlooked the importance of a proper classification, but there is a peculiar philosophical arrangement of the facts described, with the deductions drawn from them, which enables the reader to be at once acquainted with the whole of the subject, and to judge for himself of the accuracy of the author's conclusions.

M. Robin's writings all reveal a most systematic mind; from this characteristic tendency, so visible in his classification of general anatomy,* he possessed peculiar talents for the composition of a treatise on anatomical chemistry. M. Robin is an anatomist, he has prosecuted that science into its minute details, but he could not have undertaken to write alone a treatise so intimately connected with chemistry; it was therefore indispensable that this work should be the result of the united efforts of both an anatomist and a chemist, and M. Verdeil happened to be possessed of the very acquirements M. Robin stood in need of.

M. Verdeil is a chemist and a physician; his investigations have therefore been directed to chemical subjects more or less connected with physiology and anatomy. His researches into the composition of the blood, the constituents of the lungs, into the nature of animal and vegetable colouring principles, and more recently into vegetable humus, with Mr. Risler, suffice to show that physiology as well as chemistry is already considerably indebted to this author. We may also observe, that while he possesses a remarkable power of generalizing facts, and establishing theories on their mutual relations, this philosophical tendency is accompanied by a correct judgment and a clear understanding; thus imparting to his share of the treatise we are about to review a considerable degree of originality. This tendency, however, is decidedly reprehensible when appearing in too great excess, and it is of the utmost importance to guard against inconsiderate and hasty generalizations, not only because the facts upon which a theory is founded may be afterwards discovered to be false, but also from the difficulty of drawing correct conclusions even from well established facts.

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* See Tableaux d'Anatomie Générale, par Ch. Robin.

To conclude these preliminary observations, we may add, that both M. Robin and M. Verdeil appear to us to err in two directions; on the one hand by an unnecessary and indiscriminate desire to divide and subdivide the subject, and on the other by an inordinate tendency to construct hasty theories upon apparently well established facts. The authors, both from the new views they have brought forward upon the subject, and from their scientific and philosophical method of exposition, have created a new school, the peculiar character of which is complete independence. They consider that the methods of investigation in physiological chemistry have been hitherto misunderstood, and their task is to supply this deficiency.

At the commencement of the first volume, the reader is at once struck by the length of the introduction or *prolégomènes.* With the difficult task before them of stating new views, and opinions generally at variance with those advocated by the present standard writers, the authors had not only to introduce the subject to the reader. but also to refute such existing notions as were opposed to their theories. It was therefore of the utmost importance that the first part of the work should be devoted to an elaborate explanation of what they understood by anatomical and pathological chemistry.

After having carefully traced the boundaries of *anatomy, physiology,* and *chemistry,* and compared these sciences with each other, they conclude that chemistry has hitherto encroached upon anatomy, inasmuch as that part of the former science which is designated animal chemistry decidedly belongs to the latter. This view is discussed by the authors with the greatest minuteness; and every argument which might be adduced against their opinion is carefully weighed and refuted. Considering, therefore, animal chemistry as a part of anatomy and physiology, it was necessary to appropriate animal chemistry to anatomy, or, in other words, to show how the definite chemical compounds extracted from the animal solid and fluid substance constitute part of their anatomical elements. In anatomy, by means of the scalpel and forceps, we reduce tissues into their elements, such as fibres, nervous tubes, &c.; but these fibres or nervous tubes are not, strictly speaking, anatomical elements, for they contain other constituents quite as important to their existence as fibres or nervous tubes. For instance, if we treat the muscular fibres with water, this extract will yield definite chemical compounds, such as creatine, creatinine, &c.; these substances are anatomical elements of the muscular tissue as well as the muscular fibres or nervous tubes; only instead of the knife and forceps, we made use of water, alcohol, &c. To these definite chemical compounds, constituting anatomical elements, the authors have given the name of

Immediate Principles. They therefore define the immediate principles of the animal system, *the final products, solid, fluid, or gaseous, resulting from a well-conducted anatomical analysis of the various humours and anatomical elements, susceptible of no further division without an alteration of their chemical nature.*

The first volume is devoted to general considerations on the characters of these substances. Nearly all of them crystallize; and this constitutes one of their most important properties, since, by the mere inspection of the crystals under the microscope, we can recognise their nature at once, and be certain of their existing in a pure state. No doubt the necessity of obtaining crystallized substances constitutes one of the greatest difficulties met with in the study of the immediate principles; but this obstacle is easily overcome, if we make ourselves previously acquainted with the peculiar manipulations required in such investigations. Crystallized immediate principles may assume different forms, according to circumstances. Thus, common salt, for instance, when crystallized from urine by very slow evaporation, assumes the form of large, six-sided, perfectly transparent cubes; when obtained by the evaporation of alcoholic extracts, it generally assumes the shape of octohedra, or solids derived from that type; while, when prepared from ethereal solutions, the crystals resemble prisms with pyramidal extremities. The authors insist on the importance to the inquirer of making himself perfectly acquainted with the various forms of crystals of every immediate principle, which M. Robin has admirably illustrated in the atlas annexed to Messrs. Robin and Verdeil's treatise.

In regard to the methods employed in the preparation of the immediate principles, we must endeavour to make aqueous, alcoholic, and ethereal extracts, and to obtain the substances they contain by concentrating these extracts at a low temperature. The principles will generally crystallize at different degrees of concentration, and may be collected for their microscopical examination. It is impossible to give in a few words a general method for obtaining the immediate principles in a crystalline form; but the above, which is the most simple, ought always to be preferred, if possible. It proves, however, ineffectual in a great variety of cases.

It is much to be regretted that the mode of investigation we have just described does not allow us to make a quantitative determination of the immediate principles, as it is impossible to separate by crystallization the whole of a substance when mixed up in a solution with many others. In this case, therefore, we must have recourse to chemical analysis, and for this purpose a certain amount of chemical knowledge is quite indispensable. Thus, we can easily obtain crystals of phosphate of soda from the urine, but this will not give the quantity of phosphate of soda contained in it. It will therefore be necessary to precipitate the phosphoric acid by ammonia and sulphate of magnesia. But this operation, unfortunately, cannot lead to any correct results, as it will be impossible by such means to prevent the phosphoric acid existing in other forms as an immediate principle of the urine, from precipitating.

We have, therefore, no method for obtaining a correct quantitative analysis of the immediate principles. Besides the mode of crystallization of the two principles, the authors give several other methods for recognising and testing the nature of those substances.

Under the head of *Properties upon which depends the distinction between the different kinds of immediate principles*, the authors consider—1st, *The numerical or mathematical properties;* 2nd, *The physical characters;* the former comprising the volume and form, or crystallography, of the above principles, and the latter, their consistence, elasticity, specific gravity, &c., and finally their optical properties, such as their colour, their power of refracting and polarizing light, &c. The authors observe that the index or angle of refraction would be a very good mode of distinguishing the nature of the immediate principles, were it not that the minute volume of the crystals renders any operation of the kind utterly impracticable. The polarization of light by the crystals of certain immediate principles and not by others, affords another and more efficient mode of distinguishing these substances from each other. The limits of this notice, and the peculiar nature of the subject, prevent us entering into the optical details necessary for describing the polarizing apparatus by means of which this property may be ascertained, and we are forced to refer the reader to the original work, in which every circumstance connected with this part of the subject is minutely examined.

The authors consider, in the next place, the chemical characters by which the immediate principles may be tested. These characters include—

1st. *The chemical action of physical agents,* or the chemical action of changes of temperature upon the immediate principles, the theory of pyrogenic bodies, &c. 2nd. The chemical action resulting from the contact of bodies with each other, or the theory of solution and of combination. Finally, they proceed to consider the mechanical mode by which we can ascertain the specific characters of the different kinds of immediate principles, namely, the *microscope*. M. Robin having already published a treatise on the microscope and its uses, the authors refer the reader to his book for a description of that instrument. In the present work they insist more particularly upon the mode of preparing objects for the microscope, upon the different magnifying power to be used according to circumstances, upon the processes to be employed in the examination of the preparations, and likewise on the use of reagents under the microscope, the measurement of the angles and size of crystals, and the modes of delineating them.

The use of chemical reagents under the microscope being, in our opinion, of the greatest practical importance, we regret to see only one page devoted to this interesting subject. We have often observed a drop of ether added to a solution, viewed under the microscope, cause an instant crystallization; in the same manner, the action of nitric acid upon the smallest quantity of urea will produce on the microscope glass a crystallization of nitrate of urea. Common salt and oxalate of lime may also be distinguished under the microscope by the dissolving power of water on the former, and traces of carbonic acid can be detected by the addition of hydrochloric or nitric acid to the solution on the microscope glass. We cannot agree with the authors, when they tell us that the use of reagents on crystals placed under the microscope seldom offers satisfactory results; for we have found such means of incalculable value in a great number of cases. In the latter part of the first volume, our authors take an historical view of the study of the immediate principles. This portion of their work deserves especial notice, as it affords them an opportunity of reviewing and criticizing the various works connected with the subject they are treating, in order to advocate their own views.

Van Helmont was the first who, though admitting only one principle, *water*, extracted the solid substances it contained. He made us acquainted with the carbonic acid and carburetted hydrogen of intestinal gases, and showed the latter substance's property of burning; he also obtained from the blood an alkaline substance; and in the part of his work entitled *De Lithiasi seu ortu Calculi*, he endeavours to resolve urine into its constituent elements. Nicholas Lefévre, in 1660, admits that every organic substance is formed of five distinct principles—water, mercury, sulphur or oil, salt, and earth. At the same period Boyle showed that the principles extracted from the blood by fire were not true principles; he also observed that the vegetable juices are reddened by acids, and become green by the action of alkalies. Barbatus is the first who observed that blood is coagulable by heat, although he does not admit that the coagulable substance is a principle of the blood. Otto Tachenius considered an acid as the agent of every disease, and made some investigations into the nature of perspiration, in which he found salts similar to those extracted from urine, and which he calls urinary or microcosmic salts.

In 1682, Papin, by means of his steam digester, succeeded in extracting from bones, gelatine, salts, empyreumatic oils, and volatile alkali (carbonate of ammonia). In 1684, Robert Boyle published his researches. This writer studied blood physically and chemically, and endeavoured to extract its principles. He observes that the fixed salt of blood turns slightly green-blue vegetable colour, precipitates under the form of a white powder silver dissolved in aquafortis, and has a slight taste of sea-salt.

These analytical researches were naturally very imperfect, as distillation by a great heat was at that period the only known process. In 1771, Rouel the younger began his researches on blood, milk, and urine, showing a decided improvement in the methods of analysis he employed. He evaporated his solutions instead of distilling them, and treated the residue with various dissolving agents, as alcohol; the first, he considered an extractive matter insoluble in alcohol, and soluble in water, as a principle of urine. By the same method of investigation, he extracted from milk chloride of potassium, and a sugar which he compared to sugar-candy. In his comparative analysis of the blood of man, calf, ox, horse, sheep, pig, &c., he constantly detected the presence of the *natrum* or mineral alkali (soda and its carbonate), and showed that the amount of salts in the blood varies according to the different species of animals.

In 1775, Scheele's researches, and especially the improvement in his method of investigation, greatly enlarged the sphere of our chemical knowledge; among his numerous and important discoveries, we may record that of cholesterine, which death prevented his publishing. Fourcroy described it afterwards.

Guyton Morveau, in 1782, published the first treatise on chemical nomenclature. He admits several radicals or elements, as oxygen, so named by Lavoisier in 1778, who established also the existence of sulphur and several metals.

In 1789, Fourcroy pointed out the importance of extracting the elements of substances without having recourse to chemical decomposition. This chemist showed the spontaneous coagulation of blood, and the albuminous principle of urine. He found urine to contain mineral salts, and substances analogous to aqueous extracts; he also discovered peculiar principles in that excretion, such as the lithates; and finally established the importance of distinguishing those substances which have been since called *immediate principles*, or, in other words, which are separated immediately, without any alterations of the organized matter. These principles are:—1. The extracts, or extractive matters. 2. The saccharine matters, less abundant in animals than in plants. 3. Mucilages 4. Fixed oils. 5. Volatile oils. 6. Resins, more frequently met with in plants than in animals. 7. Albuminous substances, or those which solidify by heat; their proportion in animals being greater than in vegetables. 8. The fibrous substances, analogous to the gluten of flour. 9. Soda, lime, potash, and the phosphoric, muriatic, oxalic, malic, benzoic, lactic, sacholactic, lithic, prussic, and boracic acids. It is important not to overlook the above enumeration, as Fourcroy is the first chemist who began a systematic study of the immediate principles. In 1801, he published his elaborate treatise on *philosophical, meteorological, mineral, vegetable, animal, pharmaceutical chemistry, and chemistry applied to manufactures and economy.*

Thenard published his treatise on chemistry between 1813 and 1816. He admits Fourcroy's system of classification, but does not add much to our previous knowledge of the immediate principles.

The treatise on chemistry published by Berzelius soon after Thenard's, contains a more minute subdivision of the subject. This celebrated chemist describes the constitution of the organic immediate principles, and gives a clear insight into the phenomena of fermentation and of putrefaction. We find, however, nothing very new in this treatise on the subject of the immediate principles.

M. Chevreuil was the first who applied the notion of *species* to the study of chemistry, and thus greatly contributed to increase our knowledge of the immediate principles. In 1823, he published his researches on the animal fatty matters; and in the following year, his work on chemical analysis. These remarkable productions show a spirit of investigation which, avoiding *all* minute and unimportant details, has ardently in view great and general results. Chevreuil describes the *elementary* and *immediate* analysis as two distinct processes, and gives rules for the separation of the immediate principles from each other without altering their composition. He also first adopted the name these substances now bear.

Hunefeld, in 1826, in his treatise on 'Physiological Chemistry,' was the first who examined the formation of the immediate principles, and showed that those substances derive their origin from peculiar parts of the economy, and under peculiar conditions. He advises, for the extraction of organic substances, to use alternately the method by incineration, and that by which we obtain precipitates with alkalies and acids.

In 1835, M. Dumas, in his theory of organic substances, endeavoured to fix the exact boundaries between mineral and organic chemistry, and considered the examination of fibrin, starch, &c., as belonging to physiology. In 1837, he established with Liebig the theory of the compound radicals, in which they regard *mineral chemistry* as including the various bodies resulting from the direct combination of elements properly so called, and *organic* chemistry as including the different chemical species formed by compound substances acting as elements.

In 1838, Mulder published his theory on the protein compounds. His formula of protein is $C_{40}H_{31}N_5O_{12}$, while that of Liebig is $C_{48}H_{36}N_6O_{17}$, and that of Dumas, $C_{48}H_{35}N_6O_{17}$. Mulder's researches have proved very useful, from their having shown the presence of sulphur as an important constituent of certain organic substances.

Between 1841 and 1842, Liebig published his work on 'Organic Chemistry applied to Animal Physiology and to Pathology.' Liebig examines more particularly the chemical phenomena which take place in the economy in connexion with the production of heat, digestion, secretion, and respiration. He considers especially the immediate principles in their dynamical condition, without having previously duly insisted upon their individual nature and properties. His work is essentially chemical, as is proved by that part of the book devoted to the study of the metamorphoses of organic tissues, in the chapter intituled 'Development of the Metamorphoses by means of Chemical Equations.'

The chemists of this period explain the formation of immediate principles by the metamorphosis of substances varying from the proteic radical by containing more or less oxygen, each proportion of oxygen taken up by the organism producing a proportional amount of heat. By the absorption of oxygen, the living parts are destroyed, and eliminated in the state of inorganic combinations; all the oxygen thus absorbed by the respiration is not entirely used to effect this metamorphosis, a part of it being employed to convert into a gaseous form certain substances no longer useful to the system. From this combustion is derived the heat peculiar to the living organism.

"There exists," observes Liebig, "an intimate connexion between the conditions necessary for the development of animal heat and those required for the production of mechanical phenomena; if there is an increase of the former, the latter will increase in a like proportion." Liebig considers animal life to be generated by the reciprocal action of two forces, one of which would produce an increase, or make up for the waste, the other would cause a decrease, or destruction of matter. The increase is effected by the vital power, while the destruction results from the chemical action of oxygen.

"The oxygen dissolved in the arterial blood, combining with the various principles incapable of resisting its chemical action, generates the temperature necessary for the production of the vital phenomena."

A morbid principle is a substance, or merely any mechanical cause, which destroys the equilibrium between the waste and the supply. Any diminution in the resistance of the living parts against the cause of waste becomes a want of power to resist the oxygen of the atmosphere.

The above are the facts more or less connected with the study of the immediate principles, contained in Liebig's work on organic chemistry applied to *physiology*. The same ideas are reproduced in his treatise on *organic chemistry*; but, however high our respect for Liebig, we are bound to say, that several distinguished chemists have expressed doubts as to the entire correctness of views, which, from their extreme simplicity, have naturally found a great number of ardent supporters. For the arguments adduced against them we must refer the reader to Messrs. Robin and Verdeil's treatise; our limited space unfortunately prevents us from dwelling at greater length on this part of our subject.

In 1841 and 1842, Lehmann published the first edition of his interesting work on 'Physiological Chemistry.' This author, in his introduction, considers chemistry as a means of explaining physiological and pathological phenomena; and is quite opposed to the employment of chemical hypotheses in the study of organized bodies. We agree with Lehmann so far; but, unfortunately, he has not attained the object he had in view. The method he has employed explains this deficiency. He admits two kinds of chemistry, *mineral* and *organic;* and, moreover, a physiological chemistry which contains the science which he calls zoochemistry. He observes, "The fundamental principles of physiological chemistry must be searched for in general in organic chemistry. Zoochemistry is intimately connected with the science of physiological chemistry, and must be regarded as a most powerful means for its development." But in order to make zoochemistry the basis of physiological chemistry, each of its principles ought to be considered, not only in its exclusively chemical nature, but also in its general relations with the animal organism and its constituents. Accordingly, Lehmann admits an organic chemistry, or physiological chemistry, the basis of which is the science of zoochemistry; but where this organic or physiological chemistry begins, and where it ends, he is quite unable to determine. Thus he is unavoidably compelled to give an undue importance to the chemical point of view, and to adopt a general method very similar to that of the other authors who treat of this subject. Besides this, there is, in our opinion, a defective arrangement of the subject, as Lehmann does not describe, except for the inorganic principles, the constituents of the animal body when considered in the state in which they exist in the organism. Thus, for instance, *the lactates, the hippurates,* &c., are not described by the author as constituents of the body, but only as lactic, hippuric acids, &c.

When treating of the albuminous substances, Lehmann admits the protein theory, and describes several bodies which Robin and Verdeil do not consider as immediate principles. The third volume of his treatise, entitled *Histochemistry,* is devoted to the study of tissues, each chapter beginning by a short description of the several tissues, and then entering into their chemical examination, with its chemical study.

Between 1844 and 1851, Mulder published his essay on 'Physiological Chemistry.' He begins where Lehmann leaves off—namely, at the chemical molecular forces. Mulder's book is a treatise on general anatomy, in which the anatomical elements, the tissues, the immediate principles, and the humours, are described principally in a chemical point of view, and with a thorough knowledge of the importance of separating the groups of nitrogenized substances from the other constituents of the body.

In 1846, M. Dumas published his treatise of 'Chemistry applied to Physiology and Pathology.' Dumas admits the combustion of the substances assimilated by the body before they are eliminated from the organism. He believes that by that process the greatest portion of the fibrin, albumen, casein, gluten, gelatine, along with the fatty matters, are consumed, and that it is only the excess of those substances which is assimilated.

The last work which our authors mention in the historical part of their treatise, is the new Letters of Liebig, published in 1851. Liebig does not, however, refer in any way to the boundaries of the science he treats of, and his views are still essentially chemical. Since this time, other important works have been published, to which we shall not refer at present.

To give the reader an insight into Messrs. Robin and Verdeil's classification of the immediate principles, we have condensed the two last volumes of their treatise into the two synoptical tables which accompany this review.

VOL. II.

Book 2.—*Of the Immediate Principles considered individually.*

- **1st Class. Mineral or Inorganic Immediate Principles.**
  - I. General characters of the immediate principles.
  - II. Their characters considered according to sex, age, race, species, and morbid state.
  - III. Origin, formation, and exit from the system, of immediate principles of the 1st class.
  - IV. Functions of immediate principles of the 1st class in the organism.
    - *1st Division.*
      - I. Oxygen.
        - In the blood. In the lungs.
        - State in which it exists in the organism.
        - Pathological condition.
        - Exit of oxygen from the system.
        - Function of oxygen in the system.
      - II. Hydrogen.
      - III. Nitrogen.
        - In the blood.
        - In the lungs.
        - Its state in the system.
        - Origin and exit.
        - Functions.
      - IV. Carbonic acid.
        - In blood, lungs, intestines.
        - Its state and functions.
        - Extraction.
      - V. Carbnsetted hydrogen.
      - VI. Sulphuretted hydrogen.
      - VII. Hyposulphate of ammonia.
      - VIII. Water.
        - Volume and weight.
        - Origin and exit.
        - Function.
        - Extraction.
    - *2nd Division.* Mineral or inorganic.
      - Chloride of sodium; chloride of potassium, fluoride of calcium; hydrochlorate of ammonia; carbonate of ammonia; bicarbonate of ammonia; carbonate of lime; bicarbonate of lime; carbonate of magnesia; carbonate of soda; bicarbonate of soda, carbonate of potash; bicarbonate of potash; sulphate of soda; sulphate of potash; basic or neutral phosphate of lime; acid phosphate of lime; phosphate of magnesia; ammoniaco-magnesian phosphate; neutral phosphate of soda; acid phosphate of soda; phosphate of potash.
- **Organic Immediate Principles.**
  - I. Mathematical characters.
  - II. Physical characters.
  - III. Chemical characters.
  - IV. Organoleptical characters.
  - V. Variation of the immediate principles of that class, according to sex, age, race, species, and morbid condition.
  - Origin and conditions of the formation of the immediate principles of that class, and of their exit of destruction.
    - *1st Division.* Acid and saline principles.
      - Lactic acid; lactate of soda; lactate of potash; lactate of lime; oxalate of lime; uric acid; neutral urate of soda; acid urate of soda; urate of potash; urate of ammonia; urate of lime; urate of magnesia; hippuric acid; hippurate of soda, of potash, and of lime; inosate of potash; pneumic acid; glycocholate of soda; taurocholate of soda; hyocholate of soda; lythofellic acid.
    - *2nd Division.* Neutral principles and nitrogenized animal alkaloids.
      - Creatine.
      - Creatinine.
      - Urea.
      - Chlorosodate of urea.
      - Cystine.
      - Allantoine.
    - *3rd Division.* Saccharine and neutral non-azotised principles.
      - Sugar of liver.
      - Sugar of milk.

From a glance at the annexed table, it will be perceived that in this portion of the work the immediate principles are described individually, and with the utmost minuteness.

The method employed by the authors for the description of these substances is the following:

1. They consider the name of each immediate principle.
2. The terms synonymous with each name.
3. The definition of each principle.
4. They determine the place of each principle in the organism.
5. They state, in certain cases, the mass or volume of the immediate principle in relation to the volume of the body.
6. In several instances they mention the form which the principle assumes in the organism.
7. The period of its existence in the body.

These characters the authors term numerical or mathematical characters (*caractères d'ordre mathematique*).

8. They consider the gaseous, solid, semi-solid, or fluid state of each immediate principle.
9. Their weight, and also their comparative weight in relation to that of the body.

The above are the physical characters.

10. The properties of certain immediate principles may depend upon their being in a state of solution. Accordingly, the authors next examine the character of these substances in relation to this new condition.
11. The authors describe, as often as they consider it necessary, the nature of the chemical reactions of the immediate principles, in the economy, under the influence of physical agents, and when acted upon by chemical reagents.
12. They consider, in a few cases, the organoleptical characters, or the actions on our senses of certain immediate principles, such as that of common salt on the taste.
13. They describe the organic properties of each immediate principle, or the conditions upon which depend their physiological functions.
14. The variations of the immediate principle, according to sex, age, race, &c.
15. The physical and chemical phenomena which attend the entrance of the immediate principles into the body, and their exit.
16. The methods for the extraction of the various immediate principles are described with the greatest care and minuteness.
17. Lastly, a short historical notice accompanies the description of each immediate principle.

By this systematical method of proceeding, the authors are enabled to give a complete account of every immediate principle. In regard to the methods for their extraction, the authors not only describe with great precision the processes used by others, but also make us acquainted with several new methods, by which M. Verdeil was enabled to discover several principles hitherto unknown.

The second class of immediate principles, or the crystallizable organic principles, are next considered: the authors, according to their usual mode of proceeding, first enter into a few general remarks on the subject. They observe that the only organic character these principles possess is to concur in the formation of organized substances; they play only a secondary though indispensable part in their constitution. In regard to their formation, the greater number of these principles consist of the chemical elements of tissues, which require to assume that form for their elimination from the system. For this process, some are decomposed in the body, and pass into another state. In the embryo, for example, a part of the sugar contained in the liver is secreted by the kidneys as soon as it is formed, and is conveyed into the amniotic fluid, where it may be detected until the last period of embryonic life. (Bernard.) It also often happens that the above principles are eliminated from the body as soon as they are formed; and this may explain how it happens that they exist often in such small quantities as constituents of the urine.

In regard to the formation of the immediate principles of this class, in the system, no doubt they exert on each other a peculiar influence. This very mysterious action has been compared to the peculiar properties of spongy platinum, or ferments, which some authors have termed *catalytic* action. There are, however, certain chemical phenomena constantly going on in the organism which we are more able to judge of; as, when lactic, pneumic, uric, or hippuric acids seize upon the bases of certain saline principles of a mineral origin—a class of phenomena which, no doubt, greatly contribute to the production of animal heat.

The first division of the second class contains the *acid* and *saline organic immediate principles*, which are successively enumerated in the adjoining table. They are all of them salts or acids which burn generally without taking fire, and are insoluble or nearly insoluble in ether. These characters distinguish them from the fats, the fatty acids, and the soaps.

With the exception of lactic and pneumic acids, and inosate of potash, the amount of these principles is inconsiderable, except in the excrementitious secretions, such as urine, in the morbid products, and in the excremento-recrementitious secretions, as the bile. Their proportion in other parts of the body is very trifling.

The third division of the second class of immediate principles is devoted to the description of those that are saccharine or non-nitrogenized. A few observations on the subject will, perhaps, not be devoid of interest. The authors comprehend under this head the various immediate principles of the second class which take fire and burn with a flame, emitting a smell of burnt sugar (caramel). They are soluble in water, and possess the property of being converted by the presence of ferments, or nitrogenized substances, into lactic acid, or into alcohol and carbonic acid, according to the nature of the action which takes place.

Animals contain two kinds of sugars—the sugar of liver, or diabetic sugar, and the sugar of milk. Vegetables yield other kinds of sugar. *Grape sugar*, as far as we can tell from its chemical analysis, is perfectly analogous to that of the liver.

Sugar of the Liver.—This principle is a normal constituent of the liver, of the blood of the sub-hepatic veins, and of that part of the vena cava which is above the latter vessel, of the blood in the right side of the heart, and of the pulmonary arteries: none can be detected in the vena portæ, except when grape sugar is taken with the food; sugar is therefore secreted by the liver. M. Claude Bernard, to whom we owe this important discovery, found that during the period of digestion the liver produces a much larger amount of sugar than during the period of fasting; so that the arterial blood of an animal in full digestion yielded sugar, though none could be detected in the arterial system when the animal was fasting. Sugar is also found in the amniotic fluid and in the allantoine of the cow of four or five months old, and in those of the sheep from six weeks to two months old. It is sometimes wanting during the last weeks of the intra-uterine life. Thus, Claude Bernard failed to detect sugar in the amniotic fluid of the fœtuses of cows from six months and a half to seven months old; although he traced the existence of this principle in their urine.

In certain morbid states, as in the disease called *diabetes*, large quantities of sugar are found in the urine. Claude Bernard has also detected it in the saliva, the kidneys, the serosity of the pericardium, and in the spermatic fluid of a diabetic dog, but failed in extracting it from the substance of the central nervous system, the pancreas, and the spleen. The serosity contained in blisters and in vomited matters, according to M. Bernard, yielded sugar. Some sugar was found also in the acid perspiration of a diabetic patient.

We have no precise data in regard to the amount of sugar contained in the body; it must be very considerable in man, birds, dogs, pigs, horses, and rabbits. There is much less in the reptilia, and Bernard did not find any in the liver of the eel and of the snake. It exists in general in the body in a free state, but may also be combined with common salt.

The glucose, or sugar of the body, may either be conveyed into the system ready formed, or else be derived from the following sources:

1st. The various principles of the body may themselves yield sugar; it is not yet known whether these principles are organic or fatty. Bernard has shown that the livers of dogs or cats fed for four, five, or six months upon nothing but meat, bones, and the fatty matter which this food contained, yielded sugar, whilst none was found in the portal vein of these animals.

2nd. The cane sugar entering the portal vein by endomosis, disappears, and is transformed into grape sugar in the liver.

3rd. Glucose, or grape sugar, may enter the liver when taken into the body as food; in that case only can its presence be detected in the portal vein. Traces of glucose also evidently pass into the blood during the digestion of cooked amylaceous substances, for the chyme from the stomach to the cæcum always contains traces of that substance. A considerable part of the amylaceous matters taken up with the food pass in the intestines into the state of dextrine, which is probably one of the materials from which the sugar is elaborated by the liver.

Bernard has shown that the secretion of sugar by the liver is under the direct control of the nervous system. If the pneumogastric nerves be cut in the region of the neck, the production of sugar very soon ceases. If the extremity of the nerves connected with the lungs and liver be artificially irritated, no change occurs; but if the upper extremity be excited, the consequence is a reflex action through the spinal cord, which, following the spinal nerves, reacts upon the liver and again causes the production of sugar. This influence may be compared with the irritation in the lungs transmitted by the pneumogastric nerves—during the inspiration of chloroform, gas, or ether, for instance—when the amount of sugar produced by the liver is sensibly increased.

If, instead of irritating the lungs, we puncture the fourth ventricle of the brain or the olivary bodies, an increased action of the liver takes place, producing an excess of sugar, which is excreted by the kidneys. This excessive production of sugar, which can be obtained by an artificial irritation of the lungs, or of the superior extremity of the pneumogastric nerves when cut, or, finally, by the puncturing of the fourth ventricle of the brain and olivary bodies, may also be observed to occur in the human body under certain morbid conditions.*

Accordingly Messrs. Robin and Verdeil consider diabetes to be owing to various affections of the lungs, or perhaps also to some disease of the medulla oblongata.

Some experiments very recently performed by Dr. Harley, at Paris, and communicated to the Société de Biologie, appear to show that the nervous reflex action, upon which depends the secretion of sugar by the liver, is generated in the liver itself, by the stimulating power of the blood of the portal vein upon the hepatic branches of the pneumogastric nerves. Dr. Harley increased the exciitng influence of the blood of the vena portæ by injecting into that vein alcohol, sulphuric ether, chloroform, or ammonia, and observed, that two or three hours after the operation, the dogs experimented upon voided sugar in their urine, and remained diabetic from two or three hours to two or three days. The presence of sugar in the urine of those animals was detected by means of the double tartrate of potash and copper, and also by fermentation, so that no doubt exists as to Dr. Harley's results.

These experiments throw a new light upon the pathology of diabetes, as in a great number of cases this disease is brought on by excesses in alcoholic liquors; its nature is, however, so complex, that no serious attempt can be made to account for all its symptoms, until we have become more thoroughly acquainted with the nature and action of the intestinal secretions.

The sugar constantly produced in the organism by the liver is destroyed by the lungs, its amount in the left side of the heart being hardly perceptible, except during the height of digestion. Our authors conclude, from Bouchardat's experiments, that the sugar is transformed into lactic acid. We have ourselves observed that it is easily destroyed by a current of chlorine gas, or by heating its solution with acid phosphate of soda procured from human urine by direct crystallization. This circumstance may tend to throw some light upon the disappearance of the sugar from the action of the lungs, without having recourse to a process of oxidation. We hope the experiments we are now making on this part of the subject may lead to some useful results.

In order to show the presence of sugar in the liver, it is necessary to make a decoction of the tissue of that organ, previously minced with care. This solution, filtered through calico, precipitates the reduced oxide of copper from the double tartrate of potash and of copper, and readily undergoes fermentation. The amount of sugar is quite sufficient for its detection by M. Soleil's saccharimeter. When, on one occasion, we were endeavouring to discolour the solution, by means of a current of chlorine gas, for the purpose of examining it through that instrument, we observed that the sugar had completely disappeared, and that the solution, now become perfectly colourless, no longer acted upon the polariscope, and did not reduce the oxide of copper of Bernard's and Barreswil's fluid. The above is by far the best test for ascertaining the presence of sugar in the human body. Another method for detecting grape sugar in the dissolved state is described by M. Maumené, who moistens a piece of flannel or calico with a solution of chloride of tin, and dries it. If, in this state, it be dipped in a solution containing the slightest trace of sugar, again dried, and subsequently heated over a piece of red-hot charcoal, it immediately turns to a reddish brown, without charring, the colour being more or less intense according to the amount of sugar present. We have found this test of great value in a multitude of cases where the urine of dogs was the subject of our experiments.

In regard to the best mode for ascertaining the quantity of sugar contained in a solution, we must not forget to mention the *polariscope* or *saccharimeter*, which enables the observer, day by day, to follow the variations in the quantity of sugar contained in diabetic urine. The instrument is easy to manage after a little practice, and by means of animal charcoal the urine can always be obtained sufficiently clear for the experiment.

VOL. III.

Book. 2 (*continued*).

2nd Class (continued) *4th Divi* Fatty Principles, . . atty Acid and So ps.	I. Mathematical characters of fatty immediate principles of the animal economy. II. Physical characters of the fatty principles. III. Chemical characters, &c. IV. Organoleptical characters. V. Organic characters. Variation of fatty principles according to age, sex, &c. Origin, formation, and exit of fatty immediate principles.	Cholesterine. Seroline. Oleic acid. Margaric acid. Stearic acid. Oleate of soda. Margarate of soda. Stearate of soda. Caproate of potash, soda, and other alkaline salts of volatile fatty acids. Oleine. Margarine. Stearine. Elaierine. Steaerine. Cetine.

3rd Class. Of the Organic Substances, or Vegetable Immediate Principles.	I. Mathematical characters. II. Physical characters. III. Chemical characters. IV. Organoleptical characters. V. Organic characters. Their variation according to age, sex, &c. Origin, formation, and destruction of the immediate principles of that class. Their functions in the organism.	*1st Division.* Organic substances.	Fibrin. Albumen. Albuminose. Casein. Pancreatine. Muscosine. Ptyaline.
		2nd Division. Solid or Semi-solid Organic substances.	Globuline. Crystalline. Muculine. Elasticine. Cartilageine. Ostein. Keratine.
		3rd Division. Organic Colouring matters.	Hematine. Biliverdine. Melanine. Urrosacine.

BOOK 3.—*The Accidental Immediate Principles.*

BOOK 4.—*Principles imperfectly determined, or doubtful, and Substances not Immediate Principles.*

1st Section. Immediate Principles existing probably or certainly, but not perfectly determined.	Probable Principles of the 1st Class ...	Silica.
	Probable Principles of the 2nd Class	Acetate of soda. Leucine. Peculiar salts of dog's urine. Xanthine. Hypoxanthine. Lienine. First peculiar acid of human urine. Second peculiar acid of human urine. Hæmatoidine. Butyrine. Caprine, or Caprinim—Caproine, or Capronine—Capryline. Butyroleine, or Butrelaine. Hyrcine. Phocine. Phosphuretted fats of cerebral substance. Cerebric acid, or cerebrate of soda.
	Probable Principles of the 3rd Class	Neurine. Synovine. Lacrymine. Spermatine. Organic substances peculiar to dropsical fluids. Paralbumine. Pyine.

2nd Section.—Definite chemical compounds whose existence as immediate principles is doubtful.

3rd Section.—A few immediate principles, chemical elements, or simple bodies, whose actual combined state is unknown or commonly overlooked

4th Section.—Natural and artificial chemical compounds which are not immediate principles.

5th Section.—Substances which have erroneously been called immediate principles, being merely mixtures, or products of decomposition, or even neither chemical compounds nor mixtures.

By referring to the annexed tables, it will be observed that the first part of the third volume is devoted to the description of the fatty immediate principles. These the authors define "neutral, acid, or saline substances, soluble in ether and alcohol, insoluble, or very sparingly soluble, in water, and burning with a flame evolving carbon free from ammonia or other nitrogenized products."

Our authors next describe the condition in which the fatty matters exist as immediate principles in the various parts of the body, and enter into a minute microscopical examination of the fatty globules contained in milk. These globules are observed to be perfectly spherical, when derived from animals which yield a soft butter, as in the case of human milk; they are, on the contrary, in general polyphedral in cow's milk, the butter of which is of a more solid consistence. These globules are semi-solid, or nearly solid, in cow's milk, which is to be expected, as they contain 68 per cent. of margarine, 30 per cent. of oleine, and 2 per cent. of butyrine. According to our authors, the formation of butter depends merely upon the aggregation of these globules. They have previously been supposed to be surrounded by a peculiar membrane or envelope—but

Messrs. Robin and Verdeil consider this as a mistake, on account of the peculiar appearance of the spot caused by the pressing of fatty matters between two plates of glass.

Globules of fat are found in many other fluids, as in the prostatic and spermatic secretions. Their number is more considerable in the former than in the latter. Saliva, the synovial secretion, the mucus from the nasal passages, and the bile, also contain fatty principles which assume the form of spherical globules. Normal urine, and especially the morbid secretion, also occasionally contains globules of fat, presenting a soft fluid consistence, and a yellow colour deeper than that of milk. M. Rayer has observed, that if the urine be left undisturbed, these oily globules will rise to the surface with the ammoniaco-magnesian phosphates and the urates, which salts may be detected under the microscope. In some cases urine contains a sufficient amount of fatty matters to yield a considerable quantity of them to ether; the liquid then assumes a milky, opalescent nature, similar to that of chyle, and from thence its name of *chylous* urine.

This phenomenon has repeatedly been observed in hot countries, where urine generally contains also globules of blood and albumen. On two occasions the blood from a subject passing chylous urine was white. The addition of acetic acid to urine in this morbid state has never detected the presence of casein. The cases in which large drops of oil have been seen floating upon urine are very rare, and have only occurred once or twice after death arising from the fumes of burning charcoal; in these cases the blood found in the head, trunk, and extremities, contained masses of fluid fat.

The corpus luteum yields a very large proportion of fatty matters. The internal membrane of the Graafian vesicle contains, in its normal state, a few fatty granules; soon after the displacement of the ovum these granules increase in number and in volume, and assume the appearance of drops of oil. These are mingled with the amorphous, transparent, granular substance of the internal membrane. This circumstance prevents the minute drops of oil from congregating together, but as soon as they are pressed under the microscope glasses they escape and run into large drops.

The crystalline lens also yields a fluid substance of a pale rose-colour, soluble in ether, and which presents apparently the character of a fluid fat; but its precise nature has not yet been investigated.

In regard to the formation of the fat in the body, it may result either from the fatty ingesta, from a metamorphosis of the saccharine or amylaceous food, or from the nitrogenized food. We are unable to explain by what process nitrogenized ingesta can yield fat; the phenomenon is probably similar to that which accompanies the production of sugar in the liver, from nitrogenized food. In this case the conversion appears to take place in the liver; this, at least, is the opinion advocated by M. Claude Bernard, in the 'Gazette Médicale' of 1849. Liebig expresses a similar idea, but adds that this opinion still requires to be justified by experiment.

With respect to the extraction of the fatty principles from the tissues or solid parts of the animal body, the substance, previously minced, must be triturated in boiling water. The fatty matters will float on the surface, and collect into a hard mass on cooling. This mass is generally formed of stearine, margarine, and oleine. To separate these three principles, a sample of the fat is dissolved in boiling absolute alcohol; on cooling, crystals of stearine will appear first, and a few minutes afterwards the margarine also crystallizes; by means of the microscope we can readily distinguish these two substances one from the other. The oleine does not crystallize, but adheres to the crystals of stearine or of margarine, and may be obtained by pressing the crystalline mass in filtering paper, and afterwards treating that paper with ether.

In many cases this method of analysis, when used as a test, will prove sufficient. It is often, however, more convenient to transform the fats into soap by means of potash, and to decompose this soap by hydrochloric or sulphuric acid, when the fatty acids will float on the surface of the liquid, and solidify on cooling. This mass, dissolved in boiling alcohol, will yield crystals of stearic or margaric acid, which may be more easily recognised than stearine and margarine. We have often had opportunities of witnessing the crystallization of the neutral fats and their fatty acids, and have repeatedly observed the difficulty of obtaining them when they are mixed with a large proportion of oleine or of oleic acid. In certain cases, moreover, it is of the utmost difficulty to distinguish under the microscope stearic from margaric acid, and then we are obliged to have recourse to their fusing point: stearic acid fuses at 75°, and margaric acid at 56° Cent. A mixture of the two, fuses at temperatures proportionate to the amount of each acid present. Gottlieb has given a table of the fusibility of these mixtures, which will probably turn out to be of great practical use.

In order to extract fatty matters dissolved in animal liquids, the residue from the fluid, evaporated to dryness over the water-bath, may be treated with ether, and this solution mixed with alcohol, or concentrated at the temperature of the atmosphere, to induce the crystallization of the fats. This method, however, fails in every case where the fat is mixed with other principles, also soluble in alcohol and ether. To obviate this difficulty, it is advisable to treat the solution with lime or sulphate of lime, which precipitates the fats, in some cases by an apparently mechanical action, and in others, by the formation of a soap of lime. This lime precipitate, collected on a filter, then washed and dried, will yield to ether or alcohol the fatty substances it contains. When a soap of lime is formed, it must be treated first with a mineral acid and then with ether; it is by this mode that we have ourselves succeeded in detecting the presence of free fatty acids in the blood.

Much more might be added on the study and extraction of the fatty animal matters, were it not that the limits of this notice prevent us from dwelling at greater length upon this part of the subject.

On the immediate principles of the third class.—Organic substances, or coagulable principles.

These substances the authors define:

> "Fluid bodies, having the property of coagulating by heat at about 50° or 75° Cent., and also by the action of reagents; or semi-solid and solid bodies susceptible of softening, not crystallizable or volatile, or without undergoing decomposition; of an indefinite or undetermined elementary chemical composition; burning with little flame; evolving ammoniacal empyreumatic products having a sour smell; and finally, leaving a bulky, bright, porous charcoal, difficult to incinerate."

By referring to the adjoining table, the reader will at once make himself acquainted with the principles of this class. From the definition of these immediate principles, in which the authors avoid alluding to proteic compounds, we infer that they are not advocates of the proteic theory. This theory has been so much discussed of late, that we do not deem it necessary to enlarge upon it at present; the more so that recent investigations upon the albuminous compounds tend to show that it is no longer tenable.

Among the various properties of albumen, there are one or two more peculiar than the rest, which we shall now proceed to notice. The way in which albumen gelatinizes with strong acetic acid has been thoroughly investigated by Lieberkühn. An interesting reaction with acetic acid is the following. If white of egg be mixed with twice its volume of distilled water, and filtered through calico, in order to separate the precipitated mucus, a clear fluid is obtained, which does not coagulate on the application of heat, but merely turns opalescent. By the addition of acetic acid to the clear solution of albumen, no change occurs; but if a drop of that acid be added to some of the fluid after it has been boiled, coagulation instantly ensues. An excess of acetic acid redissolves the coagulum. Dr. E. A. Parkes has made the following observation on the coagulating property of albuminous fluids—viz. that if a solution of albumen in water be boiled with acetic acid, no change occurs; but as soon as a solution of common salt is added to the mixture, the liquid coagulates, and an excess of salt does not redissolve the coagulum. We have frequently repeated this experiment, and have found it an excellent method for testing the presence of albumen in albuminous urine. On mixing a very small quantity of albumen of the egg with healthy urine, we have also satisfactorily detected this substance by the above-mentioned test.

Coagulated albumen is insoluble in distilled water, either cold or hot. It dissolves, however, in that liquid, if its temperature be raised to a sufficient height above the boiling point in a closed tube, with a proportional increase of pressure. The experiment is made in the following manner:—Heat is applied by means of an alcohol or gas lamp, placed under a copper cylindrical air-bath, to which is adapted a movable cover, in which are two round apertures, one for a thermometer, and the other to allow for the expansion of the air. Under the cover of this air-bath are one or two hooks, to which is suspended an hermetically-sealed glass tube containing a mixture of coagulated albumen and water. Care must be taken that the tube does not touch the sides of the cylinder. The temperature of this case is to be raised to between 150° and 200° Centigrade, and at the end of four or five hours the albumen contained in the tube will be found entirely dissolved. In this state it has lost its property of coagulating by heat, though it is still precipitated by reagents. We are quite at a loss to explain the rationale of this experiment.

The third class of immediate principles includes the colouring or coloured organic substances.*

We have already noticed, in a preceding number of this journal, Dr. Harley's mode of extracting the colouring matter of urine, and shall proceed at once to describe the method employed by M. Verdeil for the extraction of *hæmatine*, or the colouring principle of blood. This fluid is first coagulated by heat, and the pressed coagulum afterwards boiled with alcohol, and mixed with a few drops of carbonate of soda, for the purpose of increasing its alkaline reaction. The alcohol, which has assumed an intense red colour, is then filtered, mixed with milk of lime, or pounded lime, and again boiled until it has become entirely discoloured. The precipitate, which has now acquired a green colour, is collected on a filter, in order to be treated with hydrochloric acid. The result of the operation is a thick red mass, which is dried on a filter, and introduced into a glass flask. This substance, treated with a little ether, yields to this fluid its fats, together with a little colouring matter. When entirely free from fat, the colouring matter is dissolved by boiling alcohol, and as soon as this alcohol has become cold, it is mixed with ether. Various substances dissolved in the alcohol are now precipitated, and the filtered mixture of alcohol and ether contains the pure colouring matter of the blood. This is distilled and mixed with a certain amount of water, when the colouring matter precipitates in the form of a brownish-black powder, which must be thoroughly washed with water.

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* See the Tables.

The hæmatine obtained by this process is perfectly free from fat, and entirely soluble in ether and in boiling alcohol; it differs therefore from that obtained by Lecanu by another process, and which he found to be insoluble in alcohol and in ether.

M. Verdeil employs a similar method for extracting the colouring matter of the bile, or *biliverdine.* Messrs. Verdeil and Harley have obtained *melanine,* or black pigment, from a melanotic tumour by the action of dilute aqua potassæ, a black powder being left behind which yielded by incineration more than 1 per cent. of oxide of iron mixed with traces of other salts. Indeed, M. Verdeil has observed that colouring matters are constantly combined with iron. Their state in this combination must be very peculiar, as they become entirely soluble in ether.

The latter part of the third volume is devoted to the description of the accidental immediate principles, and those of doubtful existence. It opens to the investigator a wide field of inquiry, which, carefully followed up, can hardly fail leading to interesting discoveries. This part of the treatise is moreover valuable, as it shows the mistakes fallen into by many chemists, who have too hastily and prematurely assigned the rank of immediate principles to substances which a more thorough investigation has proved to have no claim whatever to be considered as belonging to this important portion of organized matter.

William Marcet.

REVIEW VII.

1. *De la Prostitution dans la Ville d'Alger depuis la Conquête.* Par E. A. DUCHESNE, Chevalier de la Légion d'Honneur, Docteur en Médecine, &c.—*Paris*, 1853. pp. 231.

Prostitution in the City of Algiers since the Conquest. By E. A. DUCHESNE.

2. *Die Berliner Syphilisfrage.* Von DR. S. NEUMANN, Vorsitzendem des arztlichen Comités des Berliner Gesundheitspflegevereins.—*Berlin*, 1852. pp. 74.

The Berlin Syphilis Question. By DR. S. NEUMANN.

3. *Die Prostitution in Berlin, u. s. w.* Von DR. FR. J. BEHREND.—*Erlangen*, 1850. 8vo.

Prostitution in Berlin, &c. By DR. FR. J. BEHREND.

MANY, on turning over these pages, we feel assured, will ask, to what purpose so lengthy a review of a subject rarely brought before the British medical public? Why occupy so much space with an analysis of works, the chief interest of which may appear to be local? We feel, then, in some degree called on to preface this article with a declaration of the opinion, that few subjects have such intimate moral and physical relations with the well-being of man, and demand more earnestly the attention of the physician and the statesman.

Prostitution appears to have existed from time immemorial, and has had for many centuries a disease connected with it, whose baneful effects are not limited to an individual, to a family, or to a generation; effects which are readily propagated, widely spread, yet with such difficulty eradicated, that they are too often the indirect causes of destroyed physical and mental health, of premature death, of lunacy, idiocy, and suicide.

There are but few diseases which have passed through so searching an investigation, or whose treatment has formed the subject of so much debate, yet to the possibility of the prevention of which, attention has been in this country so little directed. Some unaccountable oversight or equally inexplicable dislike, has, with rare exceptions, caused even medical men to avoid dealing publicly and boldly with this subject. The necessity of removal of physical nuisances has with some difficulty been forced on our attention; while moral filth is allowed to reproduce itself to such a degree, that its progress must be checked, or generation after generation will suffer for our apathy.

The practice of prostitution both produces and propagates disease, and it is on this plea we solicit the attention of the profession to the subject of this review. We endeavour neither to excuse nor defend prostitution, but as to our profession is entrusted the duty of attending to the physical well-being of man, we are obliged to take man as we find him, and endeavour to obviate, as best we may, the effects of a cause that theologians and legislators have failed to remove. We therefore purpose using the space allotted to us, in discussing the justifiability, and demonstrating the means used on the Continent for limiting the propagation of syphilis.

"In order to limit the extension of syphilis, we must either prevent illicit intercourse, or use means for immediately discovering when the disease exists. There are but few whose ignorance of the organization of man is so great as to permit them to indulge in the utopian belief, that it is possible, by Acts of Parliament, to prevent the illicit intercourse of the sexes. However, the experiment has been tried. In Denmark, for example, such enactments as the following have been made:—[In the Danish Code, under Chris. V., Book vi., Chap. 13, Art. 30.] Any males found in brothels, &c., are ordered, for the first offence, to be punished with eight days' imprisonment; for the second offence, with double that punishment. Any woman found in a brothel is ordered to be flogged, or confined in the Spinning-house. By Art. 5, it is enacted that the owner or keeper of a brothel shall be flogged, and sent out of the province in which he resides. By magisterial proclamation, dated Copenhagen, 27th July, 1728, it was enacted, "that no soldier or sailor should have in his house or service an unmarried woman;" and by royal order of 23rd November, 1725, no publican was allowed to have more than one female at his bar, and she must have attained the age of twenty-four years. These, and a long list of other enactments, of so stringent a character, that they must have interfered with the liberty of not only women suspected of prostitution, but also the community generally, were enforced by order dated 4th April, 1809, and again in August 29th, 1829. Is Copenhagen, then, a model moral city?—does prostitution exist in the kingdom of Denmark? It not only exists, but with the foregoing regulations still on its statute books, prostitution is legally tolerated, and the extension of syphilis is provided against in a manner that will be hereafter shown.

But let it be for a moment supposed that it were possible to enact and enforce measures that would effectually prevent public or open prostitution; as a sequence, private or clandestine prostitution, a far more demoralizing evil, would be proportionately increased. All who have thought seriously on our social system must have been surprised to find how large a mass of illegalities or of immoralities are overlooked, in order to keep society together. Man's weaknesses or faults cease to be seriously dangerous to society as soon as they are known. The more secretly crime is accomplished, the greater the danger; and any enactment that tends to necessitate secrecy, endangers society more than noon-day crime.

With the admission, then, that it is impracticable, and perhaps even not desirable, to prevent public prostitution, it appears to us to be the duty of the profession to recommend, and of the government to legalize, measures whereby this necessary evil can be kept within certain limits, and by which the origin of disease can be discovered, and its propagation prevented. Disease of this class cannot be detected without an examination, which cannot be enforced without a control over those whose habits dispose them to infection; and to carry a control into effect, a registration of prostitutes is necessary. It may be objected, that the legislature cannot interfere with the personal liberty of any subject, so long as the individual does not act contrary to the laws of the realm, and thereby endanger the well-being of society. In this, as in many other cases, society may be endangered without the breach of any existing statute; were it not so, our judicial system would have arrived *at perfection; and if it be admitted that prostitution belongs to this* category, there can be no doubt of the necessity for an enactment against it. The details of a control over prostitution need not form the subject of separate articles of a bill, any more than the Commissioners of Sewers require a new clause for each clearage. The object would be completely accomplished by its being enacted that prostitution, meaning thereby the demanding or receiving of money for sexual intercourse, is a criminal act; and that as a punishment, the individual shall be placed under the control and *surveillance* of a commission, and the commission be authorized to make such arrangements as may be considered necessary for the public safety. It would be premature to enter more fully into a description of the system of control that might be adopted in this country; suffice it that we can, when called on, bring forward a plan whereby the civil liberty of the woman may in a great degree be preserved, and the public be at the same time protected.

Finally, taking our stand on the high principle, that whatever is likely to conduce to the happiness of man deserves to be heard by man, we enter on our description of what has been done on the Continent to prevent the propagation of syphilis.

The control of Prostitution in Berlin.—Under the chief of police and by the advice of Dr. Behrend, there has been formed a commission for moral police (*die Kommission für Sitten-Polizei*), consisting of—1st, The chief of police as president; 2nd, The medical counsellor of the central police board; 3rd, The chief physician for sanitary police; 4th, The chief physician for moral and humane police, under whose immediate direction comes whatever relates to prostitution, &c.; 5th, Ten physicians, to each of whom a part of the city is assigned, whose duties are to examine the women living in the brothels in their districts, and to attend in rotation every day for two hours at the office of the commission, in order to examine the women who present themselves; they are also called by the police to all cases of severe accidents, violent deaths, &c., and attend the police gratis; 6th, Four surgeons to assist the physicians.

This commission has divided prostitution in Berlin into two kinds, the tolerated or public, the non-tolerated or secret, and to the first of these we will now direct attention.

Tolerated or Public Prostitution.—Whenever any one desires to open a house for the reception of prostitutes, application is made at the office of the commission for a copy of the 'Request,' which is to be filled up, signed, and returned to the office; and as this document contains some of the most important regulations, we give a complete translation of it:*

"I request from Commission for Moral Police, permission to let in No. —, in — street, furnished rooms to women who live by prostitution. If this request be granted, I hereby bind myself to fulfil the following conditions:

"1. I shall consider this permission as a concession which the commission can at any moment withdraw or modify, without my having the right to inquire their reasons for so doing.

"2. I will not admit any woman into this house without having received, for her in particular, the official form of permission from the commission; nor will I allow any other persons excepting the women for whom I have received such permission to live therein; and if I act otherwise I shall pay to the commission 5*l*.

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* These and following regulations are not admitted into general circulation, and we are indebted to the kindness of Dr. Behrend for copies of them.

"3. I promise not to have any other than women servants, and not to employ as a servant any one who has not attained forty years of age, under a fine of 7*l*. 10*s*.

"4. I promise not to allow any woman or any man under twenty years of age to enter this house, under a fine of 7*l*. 10*s*.

"5. In the aforesaid house there shall be no noise or tumult whereby the neighbourhood may be inconvenienced; if I have given rise to such noise, or if it appears that, in the event of its being caused by others, I did not do everything in my power to prevent the same, I shall pay a fine of from 15*s*. to 15*l*., besides remunerating in full for all damages that may have been made during the tumult.

"6. I promise not to keep any spirituous drinks in this house, nor to allow any to be brought into it, nor to suffer any dancing or music therein, under a fine of from 15*s*. to 7*l*. 10*s*.

"7. I promise that the street door shall be kept shut during the day and night, and if it be at any time found open I will pay a fine of 15*s*. to 30*s*.

"8. I promise that the windows shall be left and retained in the condition which is ordered and approved of by the commission; and I will pay a fine of from 15*s*. to 30*s*. for every arbitrary alteration or neglect of these arrangements.

"9. I promise not to make any alteration in the interior or exterior of this house, without previously acquainting the commission and obtaining their permission to make it, under a fine of 15*s*. to 7*l*. 10*s*.

"10. I promise that none of the women who live in this house shall appear at the street-door, nor in any public garden or other place of public amusement, nor in any dancing-rooms nor public walk; and if one or more of them are seen in any of these places, whether they be there with or without my knowledge, I will pay a fine of 15*s*. to 30*s*.

"11. I promise that none of these women shall go on a journey out of the city, or on any party of pleasure, without having previously obtained the permission of the commission, and its being made as they direct; under a fine of 15*s*. to 7*l*. 10*s*.

"12. I engage, out of the agreement that is made between me and these women, to provide them with lodging, board, attendance, and clothing, all of which shall be subject to the inspection of the commission, whom I will inform of all changes made in these respects; under a fine of 15*s*. to 60*s*.

"13. I promise to have a list of prices printed, a copy of which I will give the commission; and in case of my demanding or receiving more than is therein stated, I will pay a fine of 15*s*. to 60*s*.

"14. I will not allow any of these women to incur debt for more than three pounds, under a fine of 30*s*. to 60*s*.

"15. I promise not to use any bodily punishment with these women, nor to confine nor use any violence towards them, under a fine of from 15*s*. to 7*l*. 10*s*.

"16. I promise not to allow any one to enter this house from one o'clock at midnight until the morning, under a fine of 15*s*. to 60s.

"17. I promise that the women shall live in all respects with, and have every right contained in, the 'Book of Regulations;'* that they shall preserve the greatest personal cleanliness, and if any of them become sick, I will immediately inform the attending physician, as well as the commission; I will especially direct my attention to the discovery of syphilitic disease and of scabies in these women; and, should either come to my knowledge, I will immediately inform the attending physician and the commission; further, I will not in such a case allow any one to visit the woman until she be examined by the physician or removed to an hospital. For any transgression of these points I will pay a fine of from 15*s*. to 15*l*., in addition to which, I will defray the expenses of any one who may have thereby become diseased.

"18. I shall inform the commission if any of these women become pregnant; and if I omit to do so, I will pay a fine of from 15*l*. to 30*l*.

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* See p. 119 for these regulations.

"19. I promise that the examination of the women and of the house can be made at any hour of the day or night by the commission, the attending physician, or police officers; that I will in every way facilitate the making of these examinations, and provide for the physician the prescribed instruments, vessels, &c. For every omission, or even neglect in these respects, I will pay a fine of 15*s.* to 60*s.*

"20. I promise to obtain from each of the women living in this house, with the exception of servants, from six to nine shillings per month,* and pay the amount half-yearly to the chief fund of the police; should any woman refuse or neglect to pay this monthly subscription, I engage to pay the same, considering her as my debtor.

"21. This is to the effect, that the monthly subscription entitles the women, when affected with syphilis, to free treatment and support in hospital, and that the owner of the house has no claim on this money.

"22. I promise that, in case any of these women are ill of any other than venereal disease, if they become pregnant, &c., I will provide them with medical attendance and support, or the commission can deduct the expenses from the security money.

"23. On the granting of this request I will pay once and for ever, to the chief fund of the police, the sum of fifteen pounds, and will not under any circumstances demand that this money be returned to me; with this one exception, that, within a half-year from the granting of this request, I be obliged from unforeseen and unavoidable circumstances to give up the permission.

"24. In order to secure the payment of the fines, I promise, within three days from the granting of this request, to deposit in the chief fund of the police the sum of forty-five pounds, as security, which is to be returned on the conditions contained in clause number 23, or in the event of my giving up this house and acting towards the women as directed, of which I will give the commission at least three weeks' notice. For this I shall not seek to have this 45*l.* returned to me, if I retain one or more of these women, and for them I shall submit to the regulations of the commission.

"25. All the above-mentioned fines &c. are completely independent of the legal punishments for offences and crimes; I am amenable to the common laws against secret prostitution, against public prostitution, imposition, secret delivery, the production of abortion, &c.; and should I, for any offence or crime, suffer legal punishment, I shall consider it as just, if the commission withdraw their permission. Further, if I thrice wilfully break the regulations of this contract, or act in direct opposition to the orders of the commission, they have the right not only to withdraw this permission, but I hereby forfeit all claim to the security money, which is, in that case, to be used for the purposes of inspection and cure.

"26. I promise to submit to the opinion of the commission on all points connected with this contract, and in case that I consider myself aggrieved by that decision, I submit to the jurisdiction of the chief of police, whose judgment shall be final; if after that I have recourse to the civil law, I thereby lose the right of retaining the permission.

"27. The commission has the right of receiving all fines incurred under the regulations of this contract, without having recourse to the usual forms of law; and I engage to raise the security money to its original amount within three days after it has been reduced by the deduction from it of the fines.

"Finally, in the event of my failing to fulfil the last condition, I hereby forfeit all claim to the forty-five pounds security."

It may not be out of place to offer here a few remarks on this form of the request. It will be observed, that these conditions are offered to the commission, for though the form has been drawn up under their direction, still it is a proposition offered to and not emanating from them, which, if acceded to, they can at any moment withdraw, and thereby close the house; while the 4th clause prevents boys or very young girls from being admitted. The 6th clause is not put in force, as beer is sold in all these houses, and there is but little difficulty in obtaining brandy and other spirits; still it is a useful regulation, as it enables the commission to punish the owner of the house if any one is proved to have become drunk on his premises; the restriction regarding dancing and music is never enforced. The regulation respecting the closing of the street door, clause 7th, is so strictly acted up to, that it is at all times necessary to ring to obtain admission, and on entering, it is immediately closed, as it matters not by whom it has been left open or even unlatched, if found so the owner is fined, and it is one of the duties of the police to enforce this regulation. With respect to the windows, clause 8th, the lower sash is firmly screwed into the frame so that it cannot be opened, while a wire-gauze screen extends half-way up the window, and renders it impossible for those within to be seen from the outside; the upper sash can be opened to ventilate the room, and the shutters are closed when there are lights within. The value of these and following regulations must be apparent; we therefore pass on to the 14th clause, in which the owner of the house promises not to allow any of these women to incur debts above three pounds, but, unfortunately, the small fine of 30*s*. to 60*s*. completely fails to ensure the fulfilment of this promise.

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* This sum is not stated in the original, but it varies between the amounts given above.

It may appear strange, that we should attach the greatest importance to the most stringent enforcement of this promise, for the public, who generally consider, or, at least, only see the one great fault in these women, often conclude, that as they are understood to have lost their character they must as a consequence have no character, except it be a bad one. Some fear, and others wish not, to look deeper than their external demeanour, lest perchance they may discover that these women are not altogether prostitute, while the majority, even of our own profession, find it much easier to allow a part of their fellow-creatures to live and sicken and die under the unrelenting scowl of society, than to study a question which society appears to have proscribed. For our part, we fully anticipate, and are prepared to meet, the opposition that any attempt to establish a system of control and protection for prostitutes will excite; but if we can prove by statistics and probabilities, that it has been, and will be attended with benefit, opposition in this as in every other discussion that has truth for its basis, will only tend to excite argument, which will, we confidently believe, establish the necessity of the system, and inevitably ensure it support.

While visiting these houses, with our kind and truly humane friend Dr. Behrend, we have asked these women why they do not seek some honourable occupation, as they can at any moment leave the house, no matter how heavily they may be in debt? [see regulation No. 1, page 121,] and the invariable answer was, "I must first pay my debts." But the debts increase, and prostitutes do not understand insolvent debtors' courts, and therefore languish on from year to year until all hope of recovery is lost. Hence, any system that has for one of its objects the reformation of these women, should enact, that it be a high, very high offence to give them credit, for as long as they can incur debt so long will they remain prostitutes, even under the most favourable system of control. But to return:

clause the 18th obliges the owner of the house, and the woman is equally bound by regulation No. 14, page 121, to inform the commission and the attending physician when pregnancy occurs. In such cases the speculum is not used after the third month, and at the expiration of the seventh she is obliged to leave the house; but as she will not be received into hospital until the termination of the eighth month, how is she to live in the interim? For this, as the state has no provision, *her fellow prostitutes come to her aid;* a voluntary subscription is made in every brothel in Berlin, and the money thus collected is given to her; on it she lives until her admission to hospital, it helps or supports her while nursing her child, or pays for its being nursed, while she returns to her wretched life to pay off old debts, and help others in her turn. With this redeeming feature in her fallen nature, it cannot justly be said that the trail of the serpent has utterly effaced all traces of the beauty and nobility of the woman's heart.

Clause No. 19 has the effect of obliging the owner to preserve complete order, and the women are most scrupulously cleanly. It is provided in the 23rd clause, that the applicant for the permission shall pay fifteen pounds to the chief fund of the police, and he is informed that the entire of this money is to be given to the institution for repentant females, while the interest of the forty-five pounds, lodged as security, is also given to the institution. Thus, the first act of one about to open a brothel is to denounce his mode of life by helping to support an asylum for repentants. It is provided in clause 25, that if the owner of the house be found guilty of any offence against the common law, he shall be doubly punished, first by the civil courts, and secondly by the permission being withdrawn, or by losing, in some cases, the security. Lastly, in order to prevent any disagreements that may arise between the owner of the house and the commission, or between the former and the visitors or the women, from furnishing interesting (?) reports for the daily press, the 26th clause endows the commission with the power of judging all complaints, and a final appeal can be made to the chief of police.

If the request is granted, the applicant gets a copy of a book printed by order of the commission, containing regulations for his conduct; but as many of these are similar to those already given in the form of request, we shall notice only the additional rules.

Regulations for the Person who is allowed to provide Lodging and Board for Prostitutes.

"4th regulation.—He shall not allow billiards, cards, or any other game to be played in his house.

"5th regulation.—A printed list of prices must be hung in each room, and the commission must be furnished with a copy. (We give the following as a specimen of these tariffs:—Entrance, 6*d.*, for which a cup of coffee is given; coffee, per cup, 4*d.*; use of a room for fifteen minutes, 3*s.*; for thirty minutes, 5*s.*; for one hour, 9*s.*; and these prices include the company of one of the women for the time stated.) If there is a higher demand made from a visitor than that stated in the tariff, on his reporting it to the commission, the owner of the house is fined in accordance to clause 13 of his contract.

"6th regulation.—The agreement between the owner and each of the women must be written, one copy to be kept by the owner of the house, a second to be given to the woman, and a third left with the commission. (These agreements are generally to the effect that the owner gets two-thirds of what moneys she gets in conformity with the tariff, and for this he provides her with lodging, board, clothing, and attendance.)

"13th regulation.—That the owner of the house must provide an examination table of a certain form, two or three specula, several pounds of chloride of lime, and for each woman, besides bed and body linen, he must furnish a washing-stand, &c., a vaginal syringe, and two or three sponges. (Each woman must have a separate bedroom, so that there cannot be more women than there are sleeping apartments in these houses; this most sanitary arrangement, though not in the 'Book of Regulations,' is invariably enforced by the commission.)

"14th regulation—If it be necessary that a woman take outdoor exercise, or if she goes out on business, she must be modestly dressed, and the owner of the house shall provide a trustworthy man to accompany her and see that she does not stand in the street or remain out longer than is necessary for her health or business.

"15th regulation—If a woman wishes and determines to leave her unlawful course of life, the owner dare not make any attempt to dissuade her from so doing, nor dare he prevent her, as soon as he is acquainted with her desire, not even if she be his debtor (The commission further enforce, that if she be entirely unprovided with proper clothes, he must furnish her with a suit such as is worn by servants, and send her, at his expense, to her native city, no matter how remote it may be)

"16th regulation.—If a woman wishes to leave this house, in order to continue her debauched life elsewhere, she must first have fulfilled the conditions in the written agreement existing between her and the owner, or she may leave in accordance with a new agreement voluntarily made by him; but of this the commission must get notice.

"17th regulation.—It is expected that the owner of the house will assist the commission in their efforts to bring these women back to an honourable course of life, that he will endeavour to prevent secret prostitution, and to trace syphilis to its origin. (It must be evident that this regulation is only useful inasmuch as it expresses the objects for which the commission has been constituted)"

The request having then been granted, and the owner provided with a copy of these regulations, the house is finally inspected, and if the arrangements are approved of, permission is given to open it.

A woman wishing to enter such a tolerated brothel must apply to the commission, with proof of her having attained the age of twenty, and being free from debt, as it is the endeavour of the commission to ensure that no one is obliged to have recourse to prostitution to free herself from debt. The regulations are read to her, she is informed that if she enters a brothel, it becomes for her a kind of prison,—in which she must submit to the regulations she has just heard; and such arguments as the nature of the particular case suggests are used to induce her to change her resolution. In the event of her adhering to her wish, she is examined, and if found healthy, her name, age, residence, birth-place, and personal appearance, are noted, so that she can be readily identified; she then obtains the written permission to enter such a brothel, and is given a book containing the following regulations, in which her name is written in full.

"*Rules for the women who have not been induced by the most urgent persuasions to leave their debauched course of life, and are therefore placed under legal inspection.*

"1st. The person can at any time leave the house in which, by permission of the commission, she obtains board and lodging, as soon as she has the earnest intention of leading a lawful and honourable course of life: nothing can oblige her to remain, neither obligations nor debts to the owner or to any other person, and in such a case she shall have the necessary protection and assistance from the commission or from the civil police of the district in which she lives.

"2nd. If a woman wishes to leave a house in order to continue her debauched life in another, she can only do so after having fulfilled the terms of the written agreement between her and the owner, or by his permission; the commission reserve to themselves the right of making exceptions to this rule, in the event of the woman desiring to leave on account of her having been bodily ill-used by the owner, or for other important and well-founded reasons; but of all such changes the commission must get notice.

"3rd. If a woman obtain the aid of the commission, and after leaving a brothel under pretence of following an honourable course of life, devote herself to secret prostitution, she shall be confined for three months in the House of Correction, and at the expiration of that period, she shall be detained there until she desires and obtains an honest employment, be given into the charge of her family, or sent to her native city.

"4th. The police of each district shall, from time to time, inquire if the women have any cause of complaint, which they or the attending physician will receive and communicate to the commission.

"5th. The woman is hereby seriously cautioned against entering into much debt, as she is responsible for all liabilities, and is thereby brought into a state of dependence which greatly increases the difficulties of her reformation.

"6th. The owner of the house must be obeyed in all that refers to the carrying out the regulations of the commission relative to the order and decorum of his house; the women must not appear at the street door or at the windows, nor attempt to attract the passers-by with words, gestures, &c.; and should any woman act contrary to these regulations, she shall, for the first offence, be punished with imprisonment for three days on bread and water, and for each repetition of the offence, with eight or more days.

"7th. That they shall not appear in the streets, or in any place of public amusement, under a penalty of three days' imprisonment.

"8th. That any necessary out-of-door exercise must be made in conformity with regulation 14th, p. 120; and she incurs imprisonment by any breach of these directions.

"9th. She shall not practise any deception or extortion against those who visit her, for which, as well as for theft, procuration, reception of stolen goods, fraud, &c., she shall be punished with more than usual severity.

"10th. That she shall preserve the greatest personal cleanliness; that during each menstrual period she shall not allow any one to visit her; that if she be in any way ill, has any swelling, ulcer, discharge, &c., she shall immediately inform the owner of the house and the attending physician. In the event of her acting contrary to this regulation, and thereby assisting in the extension of disease, she shall be sent to the workhouse for from six to twelve months.

"11th & 12th. In these regulations, she is ordered to pay particular attention to the detection of gonorrhœa, syphilis, and scabies, and is referred to the last pages of the book, where there are instructions for the detection of these diseases in both sexes, and also for the discovery of pregnancy.

"13th. This rule is a repetition of Clauses 20 and 21, p. 117.

"14th. If she suspects or knows that she is pregnant, and does not inform the owner of the house and the attending physician, she shall be most severely punished, according to the law against the concealment of pregnancy.

> "15th. After each menstrual period, she shall take a bath, or wash the entire body; and after every coitus she shall wash, and inject a solution of chloride of lime; the syringes, sponges, solution, &c., will be provided by the owner of the house.
>
> "16th She must submit to, and on no account be absent from, the ordinary examinations of the visiting physician, nor from any extraordinary examinations, which can be made as frequently as, and at whatever time, the commission may direct.
>
> "17th & 18th rules are to the effect, that the commission will act as arbitrators between the women and the owner, but that, so long as they remain in this house, they are under the control of the commission, whose regulations if they violate or refuse to obey, they shall be placed in confinement."

Having entered a tolerated brothel, the prostitute is visited twice every week by the attending physician, on which occasions the examinations are thus conducted. A woman enters the room used for the examination, gives the book of the regulations, in which her name is written, to the physician, who examines her hands for scabies, then the mouth and pharynx; lastly, the vagina invariably, excepting during the periods of pregnancy, with the speculum: her book remains with the physician. Another enters, and thus the examination is continued, finally, the physician counts the books, to see if he has examined all the women in the house, and writes his report. This is forwarded the same day, to the chief physician; and if any one has been reported diseased, she is immediately sent to hospital, which she cannot leave until a notice of her being perfectly cured has been sent to the commission.

Such are the regulations for tolerated prostitution, and the commission has most humanely enacted, that no debt or obligation can retain the women in the house as soon as they intend to enter on a proper course of life; but they do not leave, except in rare cases, and often return to their old habits; to prevent which, the third of these last regulations has been instituted. We are, however, disposed to believe, that punishment and reformation stand much less frequently in the relation of cause to effect than is generally supposed. Punishment may prevent a repetition of the act, from fear of the consequences; but in the majority of cases, the ingenuity is taxed to discover how the crime can be repeated without detection: and if this supposition be applied to cases such as the present, it will become highly probable that the most lenient measures would be most effectual in reforming the immoral tendencies of these women. True it is, that in all efforts at reform, we are often obliged to argue and work as it were backwards, directing our energies to prevent the effects, in the hope of being thus indirectly enabled to remove, or at least oppose, obstacles to the action of the cause, but it would be probably more effectual if we directed more attention than has been hitherto done to discover and remove the causes of crime. The caution in Rule 5, p. 121, against their entering into too much debt, on account of its placing obstacles in the way of their reformation, can have no effect on those who do not desire such a change. It matters not how anxious a woman may be, either to reform her life or avoid the payments of her debts, she will not venture to leave as long as she is in debt to the owner, as she is well aware, that although she can leave to-day, he can prosecute her to-morrow for debts incurred in his house. How can she meet his demand? Her most direct, easiest, and habitual way of obtaining money, is now interdicted, under penalty of a long imprisonment, as laid down in Rule 3, p. 121. The commission cannot pay her debts, or they would be holding out a premium for crime; thus she is, in point of fact, obliged to remain until she has paid her debts. There is but one means for avoiding this evil, namely, by declaring the women non-responsible for debts incurred while living in such a house; forbidding the owner, under a heavy penalty, from giving them any credit; and enforcing that all clothes worn by the women shall be provided for them gratuitously by the owner of the house. Under this or some other regulation, by which it would be rendered impossible for prostitutes to obtain credit, we might reasonably hope for the reformation of some, and the moral improvement of many.

We shall next consider—

Non-tolerated or Clandestine Prostitution.

The class of women who come under this division are those who live separately in their own apartments, and correspond to "*Les Filles Isolées*" of Paris. After having reported and brought under the control of the commission the most notorious of these women, the civil police proceed in their search, and if a woman is observed to be frequently in the street at night, dressed in a manner very disproportionate to her station in life, should information or other circumstances cause her to be suspected, an inquiry is made into her occupation, means of subsistence, those who visit her, with whom she associates, her general character, &c.; a report is then laid before the commission, and if they consider there is good reason to suspect her of living by secret prostitution, she is brought to the office of the commission and examined; if found in health, her name, age, residence, &c., are entered in the Red Book, or book of the suspected, and she is cautioned that the police know her mode of life, and, if she does not alter, she will be placed under the commission. If she comes a second time under the notice of the police, for drunkenness, &c., or if, on the first examination, she is found diseased, after being sent to hospital she is enrolled on the Black Book, or book of control, which contains her personal description and history, with the reasons for her being inscribed. She must now attend to be examined, once a week, at the office of the commission; and if she fails to be present at the appointed time, she incurs imprisonment from one day to four weeks, according to the frequence of the offence. These examinations, made by the physician that day on duty, in the presence of the chief physician, who has to countersign all orders to hospital, &c., are conducted in the following manner. On entering the waiting-room, each woman is given, by the police officer in attendance, a small book, in which her name, residence, birth-place, age, religion, size, height, complexion, colour of her hair, eyes, &c., are inserted; with this she enters the inner room, gives the book to the physician, who proceeds with the examination as stated at p. 122; he then marks her book, each leaf of which is similar to the following:

DATE.	RESULT.	SIGNATURE OF PHYSICIAN.
April 17.	H. (for healthy.)	A. B.

On leaving, she returns the book to the policeman, who, seeing it marked "healthy," allows her to depart. Meanwhile, another woman has been examined, found diseased, and her book is thus filled up:

DATE.	RESULT.	SIGNATURE OF PHYSICIAN.
April 17.	S. (for Syphilis,) or G. (for Gonorrhœa,) or Sc. (for Scabies.)	A. B.

On giving the book thus signed to the policeman, he informs her that she must go to hospital; and at the termination of the examinations, all those who have been reported diseased are conducted there under the care of one of the officers of the commission. As the women return their books to the policeman, he marks them as having attended in the registry; and should one be found to be absent, she is, on the same day, arrested and placed in confinement. If one of these women be ill, she must send information to that effect to the office; she is visited the same day by the physician of that district, and sent to hospital; but if she has feigned illness, she is forthwith arrested; and this has been so strictly acted upon, that the attendance is almost invariably regular.

Such are the regulations for public and secret prostitution in the city of Berlin. We have given a detailed, and, we believe, complete account of this system, as, after a careful study of the subject, and having seen the systems of Austria, Belgium, France, &c., in operation, we found it to be the most efficient and humane.

It may not be uninteresting here to mention the relative number of prostitutes in Berlin.

In 1849, the population of Berlin was	423,902
The male population over 16 years of age was	134,772
The number of military (not included in the above) . .	19,030
Total males	153,802
The number of tolerated brothels is now	20
The number of prostitutes in these brothels	225
The number of "non-tolerated" prostitutes under the superintendence of the police	540

If we say, then, that there are 765 prostitutes actually known to the police, this gives one prostitute to every 201 males (including the military). As, however, the total number of clandestine prostitutes is not yet known to the police, the relative proportion of prostitutes is somewhat more than this.

It is at all times exceedingly difficult to demonstrate the effects of a control such as that we are discussing, as it has been hitherto found impossible to determine accurately the amount of syphilis that exists among the mixed and migratory inhabitants of capitals. We have fortunately been favoured with a statistic of the cases of syphilis among the troops in Berlin, which fulfils all that is necessary to secure the correctness of conclusions deduced from it. It must be observed, that the number of soldiers is fixed, and that the same corps are permanently on duty in Berlin; further, these statistics have not been collected for the purpose of proving or disproving the efficacy of the system of control, as the registry of the Military Hospital has afforded the required data.

"*Report.*—To the Royal Commission for Moral Police in Berlin.

"In answer to the letter of the royal commission, dated April 30th, 1853, I have to report that, among other things, we have observed, during the last few years, a remarkable diminution of syphilis among the garrison. While in the year 1849 there were 1423 cases of syphilis among the troops,

In 1850 there occurred 670 cases.
1851 „ 526 „
1852 „ 332 „
In the first quarter of 1853 „ 59 „

Also, in respect to intensity, the disease forms a most favourable contrast with that of former years. In my opinion, the above numerical proportion furnishes the most sufficient proof of the utility of the existing sanitary regulations.

"Dr. Stumpf,
"Berlin, May 3rd, 1853. Chief Physician to the Garde de Corps."

There has been, then, a diminution of 753 cases, or more than half, on the first year, when the examination of the tolerated prostitutes or those living in brothels was made regularly; a diminution of 144 on the second year, of 194 on the third, or that of 1852, in the February of which year began the regulations for non-tolerated prostitutes; and calculating of 1853 according to its first quarter, we get a diminution of 96 cases for that year.

Between the 1423 cases that occurred in the year 1849, and the 332 that presented themselves in 1852, we have the enormous difference of 1091 cases, and we are justified in calculating, that had there been no control for prostitution the frequency of syphilization would not have diminished hence, in 1850, 1851, and 1852 there would have been 4269 cases, whereas under the control there have occurred only 1528: therefore there has thereby been saved from infection, no fewer than 2741 soldiers during a period of three years; yet a complete control has only existed for eleven months.

Let us turn for a moment to the disease in women, and we shall find, that during February, 1852, this being the first month of the inspection over the non-tolerated, about 38 women were examined every week, and the cases of syphilis amounted to 29 per cent. per month; while in April, 1853, about 540 women were examined weekly, and the amount of syphilis had fallen to 5 per cent. per month.

With facts before us such as these, the beneficial effects and direct humane tendency of a control over and examination of prostitutes, is no longer theoretical or problematical; it has been found to protect the women from the ill treatment that they almost invariably more or less suffer from the owners of brothels in Britain, while it facilitates their reformation, and at the same time protects the public health.

We postpone until the next number an epitome of the systems control used in Austria, Belgium, France, &c.; suffice it to observe, th syphilis is among the British troops *the most frequent of all diseas* about 180 cases occurring annually among every 1000 soldiers.

T. S. Holland

(To be continued.)

Review VII.

1. *Untersuchungen über Thierische Electricit* Reymond.—*Berlin*, 1848-9.
Researches in Animal Electricity. By E. Du Bois-Reymond.
2. *On Animal Electricity, being an Abstract of the Discoveries of Emil Du Bois-Reymond, &c.* *Edited by* H Bence Jones, M.D.—*London*, 185?

In the following review we have endeavoured to give a perfectly simple but at the same time faithful, account of the rise, progress, and present condition of our knowledge of Animal Electricity. If any one should think that we have descended to the enumeration of facts well known and universally admitted in the scientific world, we need only reply that our review is not intended only for those who have kept pace with the progress of investigation, but also for those whose busy lives have not permitted them to acquire accurate information on this important subject. Commencing with the simplest facts, we shall rise to the more complex, and refusing to enter into controversy, we shall endeavour to indicate the exact point to which accurate observation has reached, and beyond which it is now endeavouring to spread.

Before entering upon the subject, a few preliminary remarks on the electric current and the instruments applied to its investigation, will not, perhaps, be deemed superfluous by some of our readers.

If a strip of copper and a strip of zinc be both immersed in a glass of water, nothing remarkable occurs as long as the two metals do not touch each other, the moment they come into contact, however, an evolution of gas will be observed, which evolution is attributed to the passage of an *electric current*.

The term "current" is suggested by analogy, and is really meant to express a process regarding the real nature of which we know very little. In conceiving of and reasoning upon electrical phenomena, a physical image appears to be demanded by the intellect; and in the case before us this image is a fluid in a state of motion.

Philosophers are still disunited as regards the origin of this fluid; indeed, the question has given rise to two distinct national creeds in England and Germany. Referring to the example cited at the commencement, the Germans believe that the origin of the electric current is at the place where the two metals touch each other. It is an experimental fact, that when copper and zinc are brought into contact, and afterwards separated, the zinc is found feebly charged with positive electricity, and the copper with negative electricity. "At the place of contact of zinc and copper," says the German, "a certain force exists (the electro motive force) which decomposes the neutral fluid of these bodies, collects the negative fluid upon the copper, and the positive fluid upon the zinc; and if both metals be united by a conducting liquid, the two electricities will pass through it and decompose the liquid in their passage, thus giving rise to the chemical phenomena of the electric current."

In England, on the contrary, the general belief is, that the source of action is at the place where the liquid in the glass comes into contact with the zinc; that the current is, in fact, a consequence of the chemical relations between the zinc and the constituents of the fluid. The former theory is called the theory of contact, and was first promulgated by Volta; the latter theory, which finds in Professor Faraday its most powerful advocate, is called the chemical theory.

The partisans of both theories make use of the term "current;" but this implies a fluid flowing in one determinate direction,—down hill, for instance, in the case of water,—and here the first imperfection of the analogy between electricity and ponderable fluids presents itself. In the case of the former, the positive fluid passes from the zinc, through the liquid, towards the copper; but the negative fluid passes from the copper, through the liquid, towards the zinc, so that in reality we have two currents instead of one. To avoid confusion, however, it has been agreed upon to call that direction in which the *positive* electricity flows *the direction of the current*. Hence in our example the direction of the current is from the zinc, through the fluid, to the copper, and from the copper, across the place of contact, to the zinc, the circuit thus traversed being that which is usually called the voltaic circuit.

If, instead of uniting the copper and zinc directly, we interpose a wire between them, the action will proceed as before—instead of crossing immediately from one metal to the other, the current will traverse the interposed metallic conductor.

If the interposed wire be of suitable thickness, no apparent change will be produced in it by the passage of the current. How, then, do we know that a current is really traversing such a wire?

1. The evolution of gas in the vessel in which the strips of metal are immersed gives us intelligence on this head; if the wire be cut across, the evolution instantly ceases.

2. If the ends of the severed wire be united by a very thin wire, and the development of electricity be strong enough,—which may be secured by using several glasses instead of one,—the thin wire may be heated to redness, to whiteness, and even melted, while the thicker wire, whose ends it unites, is apparently unchanged. But the power which thus affects the thin wire is transmitted through the thick one; and this is the power to which we give the name of the electric current.

3. Suppose the conducting wire to lie north and south, and a common compass-box containing a magnetic needle to be placed underneath it; before the circuit is established the needle will be parallel to the wire; the moment, however, the circuit is established, the needle will be diverted from its parallelism and will set itself across the wire. The north-pole will point in a certain direction. If the direction of the current be reversed, the north-pole will cross the wire and point on the other side. There is, in fact, a fixed relation between the direction of the needle and

that of the current; and we possess in this instrument the most ready and valuable means of establishing the existence of the voltaic current, its strength and direction. Suppose the right arm stretched along the wire, underneath which the needle is placed, with the palm of the open hand turned downwards, and conceive the direction of the current in the wire to be the same as that of the arterial blood in the arm—viz., from the shoulder to the fingers—then the north end of the needle will point in the direction of the thumb. Preserving the wire between the hand and the needle, as in the case just described, and the palm always turned towards the wire, no matter what the position of the needle may be, whether above the wire or below it, or in a lateral position, the thumb will always indicate the direction in which the north-pole of the needle will point. By means of this little artifice, we can at once infer the direction of a current from its action upon a freely suspended magnetic needle.

Bearing this simple rule in mind, we see that if, instead of being placed beneath the wire, the needle is suspended above it, the direction of the north-pole will be different. If in the former case it was westerly, it will now be easterly. Hence, if the needle be placed between two equal currents flowing in the same direction, the two currents will exactly neutralize each other, and the needle will remain undeflected; but if the currents flow in *opposite* directions, then it is easy to see that both pull the needle in the same direction, and an increased deflection is the consequence.

The single current, however, may produce the same effect. If the conducting wire be coiled into a vertical circle, and the needle be placed within the latter, then the direction of the current in the lower part of the circumference is opposed to that in the upper, and the action is the same as that of two distinct currents in opposite directions. If, instead of being coiled *once* round the needle, it be coiled several times, the various coils being so insulated that the current cannot cross directly from one to the other, but must make the circuit of all of them, then the actions of the coils add themselves together; and, by this multiplication, a very feeble current may be made to produce a very sensible effect. On this simple principle Schweigger based the construction of the multiplying galvanometer, an instrument of indispensable utility in the investigation of feeble electric currents.

In these cases there are two forces acting upon the needle; the magnetic force of the earth tends to set it north and south, while the tendency of the current is to set it east and west, and it will approach the one or the other position according as the one or the other force is predominant. The action of the current, therefore, can only render itself evident by overcoming to a certain extent the action of the earth; and if the latter action could be removed, the effect of the current would be so much the greater. Ampère was the first to propose, and Nobili to enact, a method by which this is effected. It consists simply in taking two magnetic needles of equal strength, turning the north end of one towards the south end of the other, and connecting both by means of a rigid rod. If the needles be of the same strength and perfectly parallel, it is evident that the system is completely freed from the magnetism of the earth, which attracts and repels one end of the system with equal force at the same time, and thus neutralizes its own action. In practice, however, we are not able to place the needles perfectly parallel to each other, and the consequence is, that the system retains a slight directive action, and takes up a certain position, from which, however, the slightest force can move it. Electric currents of an infinitesimal character, multiplied in the manner already described, and brought to bear upon such a system, are at once rendered capable of observation.

It might at first sight be supposed that the greater the number of coils, the greater ought to be the action produced. In certain cases, however, there is a speedy limit to their profitable increase. True, a certain amount of action proceeds from each coil and operates upon the needle; but it must be borne in mind that each additional coil increases the resistance offered to the passage of the current, so that a point is at length attained where any increase of the number of coils would, by increasing the resistance, become positively injurious. Let us suppose the case of a current which has already passed through several miles of wire; the addition of another hundred yards will affect it very slightly, whereas the same addition to a current which has already overcome but a slight resistance may produce a very great diminution. Now the resistance of the human body is equal to that of a copper wire one millimetre thick and many miles in length, and hence, in the investigation of a current which has passed through the human body, or been generated in it, we may employ with advantage a galvanometer with an immense number of coils. A clear apprehension of this fact induced Du Bois-Reymond to construct a galvanometer containing 5584 yards, or upwards of 3 miles, of copper wire; while the thermo-multipliers of Melloni, which are used to investigate currents generated with little resistance, rarely contain more than two or three hundred feet.

In the year 1790, a lady of Bologna, possibly to lend the cheerful encouragement of her presence to his scientific labours, chanced to be in the laboratory of her husband. A skinned frog lay upon a table near to which an electric machine was in action. Once, at the moment when a spark was taken from the conductor of the machine, the frog happened to be touched by a scalpel, and the quick eye of the lady was the first to observe a spasmodic movement of the dead limbs. We feel a pleasure in assigning to the lady an honour which the best evidence on the subject declares to be her due. She drew her husband's attention to the astonishing fact. The experiment was repeated, and it was found that whenever a spark was drawn from the machine the same convulsive motion exhibited itself. At this time all men's eyes were eagerly directed towards the phenomena of vitality, and, as may be readily supposed, the discovery of a dead animal restored to temporary life by electricity created a most profound sensation. From this moment the frog was a doomed animal; the experiment was repeated everywhere, and the world rejoiced in the possession of a fact which seemed to promise the control of the very principle of life itself.

Such was the opinion of Galvani, in whose laboratory the wonderful discovery had been made. Having satisfied himself as to the efficacy of machine-electricity in producing the phenomenon, his next effort was to ascertain whether the electricity of the atmosphere could produce a similar effect. He prepared a frog, and attaching it to a *copper* hook, hung it upon an *iron* railing near his laboratory. Having watched for some time, and observing no sign of electric action, he moved the animal, but in doing so the very spasmodic action which he sought exhibited itself. He soon discovered the condition of its production—that every time the moist body of the frog touched the iron rail, a motion of the limbs was the consequence. He took the frog into the laboratory, and substituting for the copper hook and iron rail a metallic arc, found that he could produce the convulsions at will. It was only necessary to place one end of the arc in contact with a nerve, or with the spinal column, and to cause the other end to touch one of the muscles of the leg, to produce a sudden contraction of the latter. A significant fact was observed in these experiments. If the arc was composed of a single metal, the convulsions were feeble, but when one half of the arc was of a metal different from the other, the contractions were strong.

To understand the exact import and relation of these two experiments, it will be necessary to call to mind the principal laws of electric action. We know that if two glass rods, or two sticks of sealing-wax, be rubbed together with a woollen cloth and suitably suspended, one glass rod will repel the other, and one stick of sealing-wax will repel the other, but the rubbed sealing-wax will attract the rubbed glass, and *vice versâ.* This action is expressed in the fundamental law, that electricities of the same kind repel each other, while electricities of opposite kinds attract each other. For the sake of reference we will give the two electricities their conventional names, calling that developed by friction on the surface of the glass rod *positive*, and that on the surface of the sealing-wax *negative*. The foregoing law of action would then be expressed by saying that positive electricity repels positive, and negative repels negative; but that positive attracts negative, and negative attracts positive.

At the commencement of treatises on electricity, we usually find the attraction of light bodies by rubbed amber or sealing-wax alluded to. These actions, though introduced thus early, are by no means elementary. We shall find the image of a fluid very useful here. All bodies are supposed to possess electricity in definite quantity, both negative and positive, but as long as these two fluids are exactly equal in amount, they neutralize each other's action, and the body is what we should call unelectrified. If either fluid be in excess, we have an electrified body. In the cases of amber and sealing-wax the act of rubbing disturbs the balance of the fluids, so that an excess of negative fluid is left upon the wax, while an exactly equal amount of positive escapes by the rubber. A substance thus electrified possesses the power of attracting light bodies; —but how? These bodies, it must be remembered, possess their share of neutral fluid. The approach of an electrified body decomposes this neutral fluid, repelling that which is of the same name as itself, and attracting its opposite. Thus, when a stick of sealing-wax is presented to a pith ball, the surface of the ball nearest to the wax will be covered with positive electricity, while the surface most distant from the ball will be covered with negative electricity; and in virtue of the greater proximity of the unlike electricity, the ball is attracted. Before the body can be attracted it must be electrified; this is the primary act of the wax, (called induction,) from which attraction follows as a consequence.

We are now in a condition to understand the precise import of Galvani's first experiment. If a man stand upon an insulating stool with his face towards the (say positively) charged conductor of an electric machine, his face and breast, although in no immediate contact with the machine, will become charged with the opposite electricity, while his back will be charged with the same fluid as that upon the conductor. If his back be connected with the earth by a wire, the positive fluid will pass away to the earth, while the negative in front is held fast by the attraction of the conductor. While things are in this state, suppose the conductor to be suddenly removed, or suddenly discharged, by a second person. The natural condition of the man will be instantly restored, the positive fluid will rush back from the earth to combine with the negative, from which it was forcibly separated by the conductor, and this sudden re-union of the fluids is accompanied by a shock to which Lord Mahon, the discoverer and elucidator of the fact, gave the name of the *back stroke.* If we suppose a frog to take the place of the man, and the wire to be superseded by the scalpel of the student, we have the conditions of the first famous experiment of Galvani before us, the only difference being that in the latter case the contractions which accompany the shock were observed upon a dead body instead of a living one.

Far otherwise was it, however, with the second experiment of Galvani. This was a development of electricity altogether new. This is the seedling which, cast upon the mind of Volta, produced the miraculous fruits of which the world now reaps the benefit. People attribute scientific progress to chance. Arago attributes the discovery of the pile to the accident of a lady being ordered frog-broth; but this is an incomplete expression of the facts of the case. Such chances are ever present, but, like the microscopic seeds which float through the atmosphere, they perish without a proper soil. Volta at first shared the general astonishment in observing the dead limbs revivified; but he soon perceived the significance of the fact that the contractions were strongest when the conductor which united nerve and muscle was composed of two metals. Pondering upon this, he was led to reject the notion that Galvani had discovered the principle of life. "Here," said he, "we have no new force, but simply the old electricity developed in a new way—namely, by heterogeneous contact." The thought occurred to him of substituting his tongue for the frog. He placed one metal underneath his tongue and the other upon it, and, standing before a looking-glass, he brought the metals into contact. He expected to see the tongue quiver like the frog, but instead of it he remarked that peculiar taste with which everybody is now acquainted. He found that the taste was a continuous phenomenon, and hence he inferred that the electrical development must be different from that exhibited by the instantaneous action of an electric machine. He published his views, and was strenuously opposed by Galvani. The world, indeed, at first frowned upon the man who threatened to rob it of its acquired treasure. But this opposition served only to drive Volta to a deeper discussion of the subject; his was the true way of conquering a scientific foe, not by wordy warfare, but by fresh discovery; he worked until he earned the right of exclaiming, with triumphant scorn, "I don't need your frog—give me two metals and a moist rag, and I will produce your animal electricity. Your frog is nothing but a moist conductor, and is in this respect inferior to my wet rag." He had made the discovery which immortalized his name.

The Voltaic pile was constructed a year after the death of Galvani, who was thus spared the bitterness of witnessing the complete overthrow of his theory. The splendour of the new discovery dazzled the world's eyes, and perhaps prevented it from estimating the real force and value of many of Galvani's experiments. Volta, as before remarked, urged that the convulsions were due to the passage of ordinary electricity, derived from the contact of heterogeneous metals. Galvani, a man of great ingenuity, replied by an experiment wherein a single metal only was used to connect nerve and muscle. Volta retorted that the so-called homogeneous metal was not homogeneous, and that the slightest change of the surface could cause the apparently simple arc to act as if it were duplex; he showed that the immersion of one end of the arc in boiling water was sufficient to effect this. Galvani at length succeeded in producing convulsions without the intervention of any metal whatever, by causing nerve and muscle to touch directly, or by connecting them with an animal tissue. For a time Volta seemed willing to grant the existence of an electricity peculiar to the animal, though much more feeble than that developed by heterogeneous contact; he soon, however, relapsed into his old scepticism, and referred the last result of Galvani to the heterogeneous contact of the tissues themselves. In this belief he was confirmed by the discovery of the pile, which lent such a prestige to his name, that an oracular value was attached to his opinion. He bore down all antagonists. Perhaps, as in the case of some of Newton's views, the grandeur of his truth gave a currency to his error, when animal electricity was banished from the realms of science.

Alexander von Humboldt was a guest of Volta's at the time Galvani succeeded in producing contractions without the aid of metals, and, not satisfied with Volta's explanation, he undertook the repetition and extension of Galvani's experiments. He was then scarcely thirty years of age; this investigation, therefore, is one of the earliest blossoms of that genius which has since ripened into such renown. His experiments convinced him that, besides the source of electricity contended for by Volta, there was another peculiar to the animal itself. He reasons as follows:—"I skinned a frog, and prepared it so that the trunk and limbs were connected by the ischiatic nerves alone. When the red flesh of the limb was bent back, so as to gently touch the ischiatic nerves, violent muscular motions were excited. Here, then, were only two substances, nerve and muscle, organically united, brought into contact. (Volta had at first contended for the necessity of three substances, at least.) The excitation could not be attributed to mechanical pressure, for all remained motionless when the ischiatic nerve was shaken with a bit of muscle, with sealing wax, wood, and other substances. The unnatural bending of the leg may, however, be urged against the validity of the experiment. I therefore quit this uncertain way of working, and proceed to other methods. I took the two limbs of a frog which possessed a high degree of excitability, prepared the crural nerves speedily, and laid the latter, together with the whole extremity, upon a well-dried plate of glass. To an insulating handle I attached four or five cubic inches of fresh flesh, and brought it simultaneously into contact with the crural nerve and the muscle of the thigh. Strong contractions ensued. When nerve and muscle were touched by two separate pieces of flesh, no motion was observed, until these two pieces were themselves brought into contact. The above experiments were also successfully repeated with several other land and water frogs, with the small rana arborea, the lacertus agilis, and the common mouse. Here, then, were only two heterogeneous substances, nerve and muscle, in contact, and hence the idea of referring these remarkable phenomena to Volta's theory of disturbed electric equilibrium through the contact of at least three substances, must be abandoned."

The following is a summary of Humboldt's results. Strong muscular contractions were obtained:

1. When the leg of an animal was bent back against the ischiatic nerves, both being organically connected.

2. When the crural nerve and its muscle were connected by a fragment cut from the same nerve.

3. When a connexion was established between two parts of the same nerve, by means of some animal tissue.

These results were published in 1797; Galvani died in 1798; at the end of 1799 Volta discovered the pile, and for nearly thirty years silenced the supporters of animal electricity.

In 1820 Œrsted discovered that an electric current deflected a magnetic needle in the manner we have already described. Shortly after Schweigger, of Halle, acting on the suggestion of Poggendorf, multiplied the effects of feeble currents by coiling the wire several times round the needle. In 1825 Nobili imparted an unexpected delicacy to the instrument, by introducing, instead of a single needle, an astatic pair, whereby the action of the earth was nullified, and the needle left free to obey the slightest exterior impulse. The first use he made of his improved instrument was to examine with it the electric currents supposed to be developed in the nerves of animals, but without result. In the course of inquiry he was led to repeat the old experiment of Galvani, where contractions were produced without the intervention of metals. The spinal column of a prepared frog was suffered to dip into a vessel of salt and water, and the feet of the animal into another vessel; on connecting the two vessels by a piece of moist cotton-wick, contractions were exhibited. The thought occurred to Nobili that the current which produced these contractions might be detected by his galvanometer; he introduced the instrument into the circuit, but to his disappointment, although the frog was convulsed, the needle of the instrument stood still. His jealousy was excited; he had imagined that his instrument could not be surpassed, in point of electroscopic delicacy, but here he found it cast entirely into the shade. He attacked the matter once more, improved the galvanometer, and finally succeeded in obtaining a deflection of ten, twenty, and even thirty degrees from the current of the frog. This is the man who first showed the applicability of the galvanometer to rches of this nature. Nobili's success did not convince him that the current was produced by the vital actions of the animal. The opinion of Volta may have had some influence upon him, and his own failure to obtain currents from the nerves, served, perhaps, to confirm his scepticism. Five years previously Seebeck, of Berlin, had discovered thermo-electricity; he had found that if two bars of different metals be soldered together, and the free ends connected by a conducting wire, on heating the place of junction of the bars, an electric current is developed. Nobili referred the current which produced the convulsions of the frog to a thermo-electric origin, and made some ingenious experiments in confirmation of this idea. A difference of temperature, he contended, was established by the quick cooling of the nerves, on account of their comparatively small size, and this difference, where nerve and muscle were connected, gave rise to a thermo-electric current.

Nobili's conclusion had some effect in retarding the progress of inquiry on animal electricity. His theory was accepted by many eminent men, until at length its insufficiency was generally admitted to be proved by an investigator, whose name has ever since been closely associated with the progress of the science Matteucci showed that the current could be produced under circumstances where all idea of a difference of cooling was confessedly excluded. It was only necessary to immerse the legs of the whole frog, deprived of its skin, in one vessel of water, and to cause the head or back to touch the water in a second vessel; when both vessels were connected by a moist conductor, convulsions were obtained. Passing over the earlier memoirs of M. Matteucci, we will converge our attention upon a valuable paper published by him in the 'Annales de Chimie et de Physique,' for 1842, and from which we may infer the precise state of the question at this period. The memoir is preceded by a brief description of the methods of experiment. To the two ends of the wire of a galvanometer two plates of platinum were soldered, and these plates were caused to dip into two small glasses, containing a solution of sea-salt. Precautions were taken to cause the same amount of metallic surface to be in contact with the fluid, for a variation of the surface would, with a sensitive galvanometer, inevitably produce a current. The frog was prepared in the ordinary manner, it was cut across about the middle of the spinal column, deprived of its skin, the entrails and the bones of the pelvis were removed, until the animal was reduced to a morsel of spine united to the limbs by the nerves alone. The bit of spine and a portion of the nerves were plunged into one of the small glasses before alluded to, and the legs were caused to dip into the liquid of the other glass; the two thighs thus formed a bridge across the space which separated the two vessels. On establishing the connexion, the needle of the galvanometer was always observed to move, and with a frog of average vigour the current produced a deflection of four or five degrees. The direction of the current, in the frog itself, was always from the muscle to the nerve, or from the feet of the animal towards its head. The discovery of this fact is due to Nobili, who named the current "la corrente propria della rana," rendered in the paper before us, "le courant propre de la grenouille." With a very active frog a deflection of eight or ten degrees was obtained by M. Matteucci, and when his needle was more perfectly astatic, the deflection amounted even to fifteen or twenty degrees. The removal of the pelvis is by no means necessary to the production of the current; the effect is stronger if it be permitted to remain.

Our readers, doubtless, remember the arrangement of Volta's *couronne des tasses.* If a rod of zinc be soldered to a rod of copper, end to end, and the bar thus formed bent into an arc, so that one end may dip into a glass of salt and water, and the other end into a second glass, containing the same liquid, and if a number of such vessels be so connected that each glass shall contain the zinc of one arc and the copper of the succeeding one, then on uniting the two extreme glasses of the series by a conductor we obtain an electric current. M. Matteucci arranged a number of frogs in a similar manner, causing the feet of each frog to dip into one glass and its bit of spine into the next; and in this way he obtained increased effects. Dispensing with the glasses, he placed a number of frogs upon the same insulating surface, so that the nerves of one touched the limbs of the succeeding one; on connecting the ends of the series an increased current was observed. When one frog produced a deflection of four or five degrees, three or four, arranged in this way, produced a deflection of fifteen to twenty degrees. This is an important result. It might be argued that the current observed in the case of a single frog is due to the chemical action of the salt and water upon the animal parts immersed in it, but here we find that while the parts immersed, and consequently the chemical action, remain the same, the interposition of a greater number of frogs gives a greatly increased current. But if the current were due to the cause just mentioned, the interposition of more frogs would simply increase the resistance of the circuit, and thus enfeeble the current, instead of increasing it. The frog itself was also employed, instead of the galvanometer, for the detection of this current. In these experiments it was convenient to have a considerable length of nerve; hence the limb of a frog was prepared after the manner of Nobili, the thighs being cut away, and the leg alone permitted to remain, with a long filament of nerve attached to it. The pile of frogs being arranged as already described, instead of connecting the two extreme glasses by the wire of the galvanometer, they were brought sufficiently near to each other to be united by the long nerve of the prepared limb; the end of this nerve was caused to dip into one glass, and another point of the same nerve into the other. The moment the circuit was established or interrupted, the limb was convulsed. The direction of the current is also indicated by the limb with tolerable certainty. If the limbs contract on the closing of the circuit, and do not contract on the circuit being broken, the current, in the nerve, flows from the origin of the nerve towards its ramifications. If the contraction takes place when the circuit is interrupted, and not when it is established, the direction is the reverse.

In the course of the inquiry, M. Matteucci was led to experiment upon the legs of frogs only. He united a number of legs, so that the tendon of each lay against the severed extremity of the succeeding one; a current of equal strength with that produced by the same number of entire frogs was obtained, the direction in the leg being from the foot upwards. Cutting the thighs of the animals across, and arranging them so that the exterior muscle of one piece was in contact with the interior muscle of the succeeding one, a current was obtained which was constantly directed from the internal surface to the external one. The effect produced was greater than when an equal number of frogs were employed. From this important experiment, in connexion with the preceding, M. Matteucci infers the existence of two currents, one the current proper of the frog, and the other a current directed from the interior of the muscle to its surface. He also observed that the frog-current is enfeebled during convulsion, though later experiments led him to doubt this result.

M. Matteucci next operated upon warm-blooded animals. The reason why the frog is chiefly used for these experiments is known to be, that it preserves its vitality long after death; in warm-blooded animals the vitality soon ceases, and with it all electric action. M. Matteucci succeeded in producing contractions in the limb of a rabbit, quite similar to those produced in the experiments of Galvani and Humboldt. The nerve was separated from the thigh, raised by a glass rod, and suffered to fall upon the muscle of the leg; contractions followed. He also obtained a current on wounding an animal, and dipping one of the terminal plates of the galvanometer into the wound, while the other plate was placed upon the surface of the wounded muscle. The current was constantly directed, in the animal, from the bottom of the wound to the surface of the muscle. Finding, however, some serious irregularities when the terminal plate was brought into direct contact with the animal, he resorted to the method of piles, and arranging his cups like Volta's *couronne*, he prepared a number of pigeons' thighs, and placed them so that in the fluid of each cup the muscle of one thigh and the leg belonging to the succeeding one were plunged. Dispensing with the cups, he composed other piles, where the animal parts were brought into direct contact; piles were also constructed in which the nerve of the thigh was simply caused to touch the tendon of the leg. Other birds, smaller than the pigeons, were made use of, and rabbits were also examined. The result is, that a current was always exhibited by the galvanometer, which was directed, in the animal, from the nerve or the interior mass of the muscle to the external surface.

In a notice at the end of the memoir of which we have just given a digest, M. Matteucci communicates the following important observation. A frog was prepared after the manner of Galvani, with its lumbar nerves exposed. The leg and the long nerve which passed from it to the vertebral column were taken from another frog, and the leg was so placed that its attached nerve rested against the thigh of the frog prepared according to Galvani's method. The lumbar nerves of the latter were connected with a voltaic element; the passage of the current through the nerves caused the limbs connected with them to contract, and at the same moment *the leg whose nerve simply rested upon the thigh, as aforesaid, contracted also.* When, instead of making use of a voltaic current, the nerves were mechanically excited, so as to produce contractions, the prepared limb was also convulsed. The contractions of the muscle of a rabbit were also found to produce a sympathetic contraction in the limb of the frog. It is a significant fact that, although the muscle on which the nerve rested might be moved mechanically, no contraction of the prepared limb followed; a real muscular contraction was alone able to produce a motion of the neighbouring leg. We shall have occasion to return to this subject.

M. Matteucci deduces a number of general conclusions from his interesting paper, of which the following chiefly concern us:

1st. That in the frog and in warm-blooded animals an electric current is exhibited, when the interior of a muscular mass is connected by a conducting arc with its external surface.

2nd. That the nerve belonging to a muscular mass, and all the cerebral system, perform the office of the interior of the muscle through which the nerve is distributed.

3rd. That the current is directed in the animal from the interior of the muscle to its surface or to its tendon.

5th. In the case of the frog a current (*le courant propre*) is obtained on connecting the muscles or tendons of the fore-leg with the muscles or nerves of the thigh; this current is directed in the animal from the leg to the thigh or nerve.

7th. It remains to be explained, and to the anatomist, perhaps, must be referred the solution of this question, how, in the case of the frog, the muscles of the leg, and particularly the tendons by which they are terminated, play the same part in the production of the *courant propre*, as the interior of the muscle, or the nerves distributed through the muscles, in the case of warm-blooded animals.*

In January, 1843, a month or two after the appearance of M. Matteucci's memoir, a remarkable paper bearing the following title was published in Poggendorff's Annalen :—'Preliminary abstract of an Investigation on the so-called Frog-current, and on Electric Fishes; by Emil du Bois-Reymond.' The author does not describe in detail his methods of experiment, but he announces in distinct terms a law which casts a flood of light over the complicated phenomena which M. Matteucci was the first to observe. The law, which is as simple in its expression as it is embracing in its application, may be stated as follows:—"*When any point of the longitudinal section of a muscle is connected by a conductor with any point of the transverse section, an electric current is established, which is directed, in the muscle, from the transverse to the longitudinal section.*" Let us apply this law to the 7th conclusion of M. Matteucci, where he refers to the anatomist the solution of the entangled problem of the *courant propre.* Connecting the tendon with the muscle of the thigh, we have a current. Now the transverse section of the muscular fibres abuts against the tendon; the latter is a conductor, and hence when we connect the tendon with the thigh, we, in point of fact, connect the transverse section of a muscle with the longitudinal section. By removing the tendon, we simplify the connecting arc, and the current is observed as before. We thus arrive at the important conclusion, that the frog-current, instead of having a distinct individuality assigned to it, ranges itself naturally under the general law of muscular currents, and thus the difficulty which it presented receives the most complete solution.

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* A sense of justice to M. Matteucci induces me to mention another paper of his published in 1843, subsequently, and without doubt altogether independent of that of Du Bois-Reymond. This paper describes the effects of poisoning, of temperature, and various other circumstances, upon the strength of the muscular current, and is replete with interest. The experiments appear to be judiciously varied, the reasoning is clear, objections are stated with frankness, and met with ability.—*J. T.*

The experiments on which the law of the muscular current is based were made on the frog, on pigeons, on rabbits, on the water-crab, and on lizards. The electro-motive action of the nerves, concerning which we had been hitherto in total darkness, is also stated in the same paper. The action is precisely the same as that of the muscles; if the transverse and longitudinal section of a nerve be connected, we have a current in the same direction as in the case of muscles, and differing from the muscular current only in the fact of its being feebler. We do not hesitate to express our opinion that M. Matteucci's method of experimenting with piles of muscles, possesses advantages peculiar to itself; it certainly enabled him to exhibit an increased action, and this fact conducts at once to the important inference that the current cannot be due to the action of the liquid in which the extremities of the pile were immersed. If we seek the elementary cause of the phenomena, however, we cannot resort to such piles; they by no means represent the matter under its simplest form, and the consequence is, that the law of action deduced from them by M. Matteucci would, under certain circumstances, totally fail. In all the experiments of the latter philosopher on wounded animals, we have no indication of the *direction* in which the incision was made, nor does the slightest importance appear to be attached to this capital condition. If, however, we arrive at the interior of a muscle by an incision parallel to the fibres, the law affirmed by M. Matteucci, that a current is always obtained on connecting the interior and the exterior of a muscle, breaks down. Again,—the tendon, as before stated, is the moist conductor against which the ends of the muscular fibres abut. Let us suppose the incision to be made parallel to the fibres; then, a point of the interior muscular mass being connected with the tendon, or with the natural bases of the fibres, would show a current from the external surface to the internal; which is directly opposed to the requirements of the law of Matteucci. In arguing thus, we take it for granted that Du Bois-Reymond's statement of the law is correct, and our object is simply to show that his statement differs essentially from the statement of M. Matteucci. We take Du Bois's law for granted, because we are not aware that it has been denied by M. Matteucci, and it further comes to us recommended by the authority of a committee of the Academy of Sciences. The point at issue is, not whether the current observed by Du Bois be the same current as that previously observed by M. Matteucci, for this is indisputably the case, but whether the law affirmed by Du Bois be the same as the law affirmed by M. Matteucci, which is indisputably not the case. M. Matteucci experimented with limbs, and portions of limbs, and obtained results confessedly important, but Du Bois shows us that not only the separate muscles which compose these limbs, but the separate fibres which compose these muscles, are the real seats of the electro-motive action.

In later experiments, the author last mentioned expanded the law of action above expressed, and proved, that to obtain a current it was not absolutely necessary to connect the longitudinal and transverse sections; that, under certain circumstances, on uniting two points of the longitudinal section, or two points of the transverse section, a current, though much feebler than that resulting from the connexion of the two different sections, is obtained. Let a cylindrical piece of muscle be imagined, the fibres of which are parallel to the axis of the cylinder; suppose the length of the cylinder to be bisected, and call the point of bisection *a*, then, if two points at opposite sides of *a*, and equally distant from it, be connected, we have no current; but if the distances from *a* be unequal we have a current. This is the case when the point *a* itself is connected with any other point of the cylindrical surface. In like manner two points of the transverse section, equally distant from the axis of the cylinder, on being connected produce no current; but if one point be more distant from the axis than the other, a current is obtained.

Du Bois also places the fact beyond doubt, that if a muscle be tetanized, its current undergoes a remarkable diminution, while the convulsions last. Let a gastrocnemius muscle be laid upon the terminals of the galvanometer, so that the muscular current is shown by the deflection; let the long nerve attached to the gastrocnemius be irritated by a series of electric shocks, so as to throw the muscle into convulsions; the needle instantly descends, and sometimes passes to the negative side of zero. A hasty reasoner would infer that a current in the contrary direction had been excited in the muscle; but this is not necessarily the case. Let an ordinary voltameter be introduced for a few seconds into a common voltaic circuit, the liquid within the instrument will be decomposed, oxygen will discharge itself on one platina plate, and hydrogen on the other. If the circuit be now interrupted, and the two plates of the voltameter speedily united, we obtain a secondary current, of brief duration, and in a direction opposed to the primary one. On this fact (commonly called polarization) the well known pile of Ritter is based. Exactly the same takes place in the case now under consideration. The muscular current first causes the polarization of the platinum plates at the end of the galvanometer wire, and when the original current has been enfeebled by tetanus, the secondary current, due to polarization, comes into visible play and produces a negative deflection.

We are now in a condition to take up the discussion of the remarkable fact communicated by M. Matteucci—that when the nerve of a prepared limb is laid against the muscles of a frog, prepared according to Galvani's method, on causing the latter to contract, the prepared limb contracts also. To this action M. Matteucci has given the name of *induced contraction*. Its cause he has failed as yet to discover; but his view may be in some measure inferred from the name he has bestowed upon it. The fact appears to us to be one of the most interesting yet discovered in the domain of animal electricity. Du Bois-Reymond accounts for it as follows:—If the transverse and longitudinal section of a muscle be in any way connected by the nerve of the prepared limb, a current will proceed through the said nerve, from the latter section to the former. This current announces itself by the contraction of the muscle of the prepared limb on first making the contact. The contractions cease when the current is fairly established in the nerve, and on breaking the circuit they are again observed. But it is not on the closing or the breaking of the circuit alone that contractions are produced; every sudden fluctuation of the current traversing the nerve is accompanied by contractions. Applying this to the case before us, we find that the current of the muscle against which the nerve of the prepared limb rests, circulates through the said nerve. When the muscle is tetanized, this current is diminished at each convulsive effort, and its fluctuations are answered by corresponding contractions of the prepared limb. The extreme beauty of this explanation cannot fail to strike the reader. In one way alone can it be impaired, and that is by denying that the nerve touches two portions of the muscle in the manner above described; this is exactly what is done by M. Matteucci. Not having seen the experiment, we are unable to offer an independent opinion; but we may be permitted to refer to the decision of the Paris Academy, which is "that the above fundamental fact furnishes a direct explanation of the induced contraction of M. Matteucci."

Hitherto we have abstained from mentioning the manner in which the galvanometer was used in these experiments. It would never do to bring the ends of the galvanometer wire into direct contact with the animal parts; the wires are in reality terminated by plates of carefully purified platinum, which dip each into a suitable porcelain vessel containing a saturated solution of common salt. If the space between the vessels be bridged over by an electro-motor of any kind—an arc of zinc and copper, for example, whose ends dip into the two vessels—the circuit is established, and the current developed will exhibit itself on the galvanometer. To render the process of bridging more easy, bosses of porous paper saturated with the fluid dip into the vessels, and the substance to be experimented with is usually placed across from boss to boss. It is scarcely possible, by mere writing, to give an exact idea of the ingenuity and adaptability of this contrivance, and we must therefore content ourselves with this brief indication of the method pursued.

Casting our thoughts back upon the muscular current and its law of action: if it be granted that this current is developed in the muscle itself, we can scarcely fail to conclude that it is in a state of circulation during the life of the animal. We know that on connecting the transverse and longitudinal sections a current appears; but such connexion exists naturally in the animal body, and hence the inference is a fair one—that such currents are perpetually present, and that the current which we perceive on the galvanometer is in fact but one of the branches of these pre-existing currents. This premised, we are in a position to understand the important facts now to be described The porous bosses were removed from the vessels of salt and water, and a live frog was so placed that its two legs dipped into the two vessels. We know from the experiment of Nobili that a current exists in the frog, directed from the foot upwards, but in the case before us we have two such currents, one from each foot, which meet at the junction of the limbs, annul each other, and consequently produce no effect upon the needle of the galvanometer. But let us suppose one of these currents to be enfeebled, while the other retains its full strength; the result will be, that the excess of the latter current will produce a deflection. The ischiatic nerve of one of the frog's legs was severed, and the limb thus deprived of all power of motion; the animal was then poisoned by strychnia, and strong convulsions followed; the uninjured limb contracted violently, its muscular current was thereby diminished, and the current of the other limb was immediately exhibited by the galvanometer. A single step now carries us to an experiment which forms the climax of this fine inductive argument. Instead of the frog's feet, Du Bois placed the first finger of his right hand in one vessel, and the corresponding finger of his left hand in the other; but instead of cutting his nerves, as in the case of the frog, he suffered the left arm to remain at rest, and contracting the other forcibly, produced a deflection of the needle. When the left arm was contracted and the right one suffered to remain at rest, the needle was deflected in the opposite direction. The current always proceeded from the hand of the contracted arm to the shoulder; but remembering the fact that it is the excess of the current of the motionless arm which is here observed, we are led to the inference that in the normal state of the arm the direction of the current is from the shoulder to the hand.

The publication of this result created a considerable sensation; it was received by many with doubt and misgiving. Some eminent men undertook to repeat the experiment; their results were negative, and for a time the opinion was predominant that Du Bois was in error, and that M. Humboldt, who took a conspicuous part on the affirmative side of the question, had suffered himself to be misled. The fault, however, rested neither with Du Bois nor with M. Humboldt. Those who attempted to make the experiment had neglected its prime conditions, and their failure was a matter of course. The fact indeed is undeniable, whatever may be the fate of its explanation. We have ourselves repeated the experiment ten or fifteen times in the course of an afternoon, and always with the same constant result.

A most remarkable deportment of the nerves, discovered by Du Bois-Reymond, is now to be noticed. Let a long nerve be imagined, with its transverse section placed against one of the porous bosses already mentioned, and a point of its exterior surface against the other boss; the nervous current will exhibit itself according to the law already stated. Let a portion of the nerve near its free end, and entirely out of the circuit of the nervous current, be placed between the poles of a voltaic battery, and let a current be sent through it. The moment the voltaic current passes, a remarkable change of the nervous current is observed. If the direction of the voltaic current coincide with that of the nervous current, an increased deflection will be exhibited; but if the two currents are opposite in direction, the deflection is diminished. It must be carefully borne in mind that the modification of the nervous current is not due to the irruption of the voltaic current into the circuit of the galvanometer; there is no such irruption. The effect is similar to that produced upon a long bar of soft iron, one end of which is introduced into a helix, through which an electric current passes. This end will be magnetized, but the magnetism is transmitted through the molecules to the other end of the bar. A similar molecular modification must be supposed to occur in the case of the nerve, which increases or diminishes its proper action according to circumstances. To this state of the nerve the term electro-tonic has been applied, a term first introduced by Faraday, to express a state of matter which Du Bois considers to be in some respects similar to the above.

The important idea is here suggested, that the transmission of an impression to the brain is effected by a molecular change, which takes place along the line of transmission. The velocity of this transmission may be a quantity capable of accurate measurement. The important researches of Helmholtz,* indeed, prove that the moment an impression is made by no means coincides with the moment when it becomes evident to consciousness. If two portions of the skin be simultaneously excited, both excitations travel forward along the nerves to the brain; but the impression made upon the point nearest to the brain arrives first. If a harpoon be struck into the tail of a whale 80 feet long, the animal, in all probability, is not conscious of the wound for a second after its infliction; and before the command to strike can be transmitted through the motor nerves to the tail, another second probably elapses. In the human subject Helmholtz finds the velocity of nervous transmission to be about 195 feet a second, and in the frog 86 feet a second. It would be of the highest interest to examine whether these numbers do not express the speed with which the electro-tonic condition is transmitted through the nerves.

In his later memoirs, Du Bois has given us the results of an elaborate investigation of the influence of various states of the skin in the production of currents. By heating the parts unequally, by immersing the skin for unequal periods in the solution which contains the platinum terminals of the galvanometer, by stretching the skin, by removing it altogether by means of blisters, by causing it to perspire unequally, he has opened to us a series of electro-motive agencies, which while they excite our astonishment, impress us forcibly with the extreme caution necessary to the successful prosecution of researches upon this subject. As in the moral world improved means imply increased responsibilities, so here in the physical, the use of instruments of such surpassing delicacy, demands a proportionate watchfulness on the part of those who use them. But whatever the fate of animal electricity, as a distinct portion of science, may be, these investigations will remain as records of masterly experimental skill, and of consummate ingenuity in combating the million difficulties by which the subject is surrounded.

John Tyndall.

Review IX.

1. *Traité pratique des Maladies Vénériennes, contenant un chapitre sur la Syphilisation, &c.* Par S. G. Maissonneuve, M.D., et H. Montanier, M.D.—*Paris*, 1853. pp. 564.

Practical Treatise on Venereal Diseases, with a chapter on Syphilization.

2. *Traité des Maladies Vénériennes.* Par A. Vidal (de Cassis).—*Paris*, 1853. pp. 549.

Treatise on Venereal Diseases.

3. *Rapport à M. le Préfet de Police, sur la question de savoir si M. le Dr. Auzias Turenne peut être autorisé à appliquer ou à expérimenter la Syphilisation. à l'Infirmerie de la Prison St. Lazare?* Par MM. les Docteurs Melier, Philippe Ricord, Denis, Conneau, et Marchal (de Calvi).

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* Philosophical Magazine, November, 1853.

Report to the Prefect of Police, on the question whether Dr. Auzias Turenne be permitted to practise or experiment on Syphilization, in the Infirmary of the Prison St. Lazare?

4. *Syphilitic Diseases, their Pathology, Diagnosis, and Treatment, including Experimental Researches on Inoculation, as a differential agent in testing the character of these affections.* By JOHN EGAN, M.D., M.R.I.A.—*London.* 1853. pp. 346.
5. *Observations on Syphilis, and on Inoculation as a means of Diagnosis.* By JOHN CROWCH CHRISTOPHER, M.R.C.S.—*London,* 1853. pp. 74.
6. *Traité pratique des Maladies Vénériennes.* Par le Dr. PHILIPE RICORD. —*Paris,* 1851.
7. *La Sipilizzatione studiata qual mezzo curativo e preservativo delle Malattie Veneree.* De CUSIMINO SPERINO.—*Turin,* 1853. pp. 903.

Syphilization treated as a Curative Means in the Venereal Disease.

8. *Traité des Maladies Vénériennes, contenant le recit d'une tentative de Syphilisation, et de plusieurs expériences d'inoculation pratiquées sur les animaux.* Par le Dr. MELCHIOR ROBERT.—*Paris,* 1853. pp. 502.

Treatise on Venereal Diseases, with an account of an experiment on Syphilization, and of many inoculation experiments on animals. By Dr. MELCHIOR ROBERT.

(*Concluded from vol. xxiv. p.* 398.)

PURSUING our plan of investigating the morbid processes to which the syphilitic poison gives rise, we come to the consideration of its effects upon the lymphatic system.

The opinions of Hunter concerning the use of the absorbent vessels, have given a bias to all subsequent reasonings upon this subject; and the theories based upon his experiments are commonly received, even up to the present time. Hunter, assisted by his brother, and by Drs. Clayton, Fordyce, Michaelson, and others, found that when he confined some warm milk in a portion of small intestine, and tied the artery and vein which supplied it, the lacteals of the part were presently filled with the white milk. Upon puncturing the vein upon the distal side of the ligature, it was soon emptied of its blood by pressure with the finger; but no white fluid could at any period during the continuance of the experiments be seen in the vein. Upon a repetition of the experiment, in which the circulation through the mesenteric vessels was left free, the blood in the vein was carefully examined and compared with that in the neighbouring veins, but it was found not to be light-coloured, nor milky, nor could any difference whatever be detected in it. It was found that even by firm pressure (which was continued until the intestine burst), the milky fluid could not be made to pass into the veins. In another animal, some thin starch, coloured with indigo, was introduced into the small intestine, and the lacteals soon appeared to be filled with a fluid of a fine blue colour. A vein in this part of the mesentery was opened, and the blood which flowed was allowed to separate into coagulum and serum. The next day the serum had not the least bluish cast. An injecting pipe was then fixed in an artery of the mesentery, where the intestine was filled with blue starch, and all communications, both in the mesentery and intestine, were closed, with the exception of the vein corresponding with the artery. Warm milk was then injected by the artery until it returned by the vein. This was continued until all the blood was washed away, and the vein returned a bright white milk. The milk thus circulating through the intestine containing the coloured starch was not in any degree changed in colour. In a third animal, some musk in warm water was confined in a portion of intestine. After waiting a little time, some of the lacteals of the part were opened with a lancet, and some of the watery fluid which they contained was received into a small spoon. This was found to smell strongly of musk. Some blood received in a clean spoon from one of the veins of the same part, had not the least smell of musk.

From these and similar experiments, Hunter arrived at the inference "that the red veins do not absorb in the human body;" and consequently that the lymphatics were "the only absorbents." (On the Venereal, p. 253.) These premises naturally led to the further conclusion, that poisons were necessarily absorbed by the lymphatic vessels; and accordingly we find Hunter asserting "that the venereal matter is taken up by the absorbents of the part in which it is placed, and carried along the absorbent vessels to the common circulation." (*ib.* pp. 256, 257.)

This view, deriving, as it does, such an apparent confirmation from the frequent occurrence of inflamed lymphatic glands in conjunction with venereal ulcers, has been adopted, with more or less modification, by almost all subsequent writers.

The accuracy of the experiments upon which Hunter based his theory has justly been called in question by other physiologists, but the theory itself, strange to say, has hitherto scarcely been questioned. MM. Tiedemann and Gmelin, after mixing various substances, which might easily be detected, with the food of animals, not unfrequently found unequivocal traces of these substances in the venous blood and urine, whilst it was only in a very few instances that traces of them could be discovered in the chyle. In repeating Hunter's experiments, Mayo proved, that half-an-hour after a solution of starch and indigo had been placed in the cavity of the intestine, the lacteals appeared of a clear blue colour, and those present were for a short time satisfied that the indigo had been absorbed. But upon placing a sheet of white paper behind the mesentery, it was found that the blue tinge disappeared,—the vessels were simply empty. On removing the white paper, they reassumed their blue colour. Thus a repetition of the Hunterian experiments rather goes to prove that the function of the lacteals is limited to the absorption of chyle. But perhaps the most conclusive experiment on this subject is that of M. Ségalas. A fold of small intestine was drawn out of a wound in the belly of a dog. All the bloodvessels passing to and from it were tied but one large artery. A vein, punctured upon the mesentery, allowed the blood to escape, which would otherwise have stagnated in the part. The lacteal vessels and nerves were left entire. The fold of intestine was then tied at both extremities, and an aqueous solution of the alcoholic extract of nux vomica was poured into it. During the hour which followed, the poison produced no symptoms. The ligature being then removed from one of the veins, the blood was allowed to return in the natural course of its circulation. In six minutes from this time the poison took effect. The experiments of M. Magendie, illustrating the same point, in which the poison of the upas tienté was introduced into the system of a dog, through a limb which had no connexion with the body, excepting through the blood-vessels, is well known.

From these facts, it appears certain that Hunter's idea of the lymphatics being the only absorbents, is incorrect, and we are thence naturally led to the consideration of the value of the theory which was based upon that notion.

An extensive observation of cases of syphilis, will, we believe, establish the two following very important points in relation to this subject: first, that in those instances in which the irritation of the lymphatic glands is the greatest, and where, consequently, we have the best evidence that the morbid matter has entered them, there is very seldom indeed any secondary syphilitic affection; and secondly, that the best-marked cases of constitutional infection are as rarely preceded by any very evident signs of inflammation of the absorbent glands. In the first class of cases, we may trace, in the most satisfactory manner, the progress of the syphilitic virus along the absorbent vessels as far as the first lymphatic gland that it meets. In any part of this course, the poison may be arrested, and produce a fresh chancre, thereby affording unequivocal evidence of its presence. But neither experiment nor observation affords any proof that the virus is conveyed unchanged through these glands. All the evidence which we have upon this subject tends to an opposite conclusion. We find even Hunter asserting:

> "We never find the lymphatic vessels or glands that are second in order affected. When the disease has been contracted by a sore or cut upon the finger, I have seen the bubo come on a little above the bend of the arm, upon the inside of the biceps muscle, and in such where the bubo has come in that part, none have formed in the arm-pit, which is the most common place for the glands to be affected by absorption" (Op. cit. p. 260.)

In like manner, after artificial inoculation, we may trace the poison, in many cases, along the lymphatic vessels, as far as the first absorbent gland which they enter. Here, however, some change is produced. Beyond this the specific characters of the poison can no longer be detected, either by its natural effects, or by artificial inoculation. After the virus has undergone digestion or concoction in a lymphatic gland, we no longer have evidence that it is capable, either locally or constitutionally, of producing its specific effects. The grounds upon which it has been assumed that the syphilitic poison enters the system through the absorbent vessels, we therefore consider most inconclusive. The usual mode in which the system does become infected by syphilis, may, we believe, be traced in another and a much more satisfactory manner.

In the development of primary syphilitic disease two processes may distinctly be recognised: one, that by which the surrounding tissues become indurated; the other, by which the same parts are ultimately removed. This second result may be accomplished either in the natural process of growth, by ulceration, by sloughing, or by different modifications of these. But beyond the parts which are involved in these processes other actions are going on, of a more subtle nature, and not so easily *appreciable by our senses. In the absence of more positive knowledge, we may ascribe these to the molecular changes in the nutrition of the* parts surrounding a chancre. That such actions are in active operation beyond the sphere both of the adhesive and ulcerative processes may be readily demonstrated, although we may be unable to define their exact nature. Were this not the case, we should have nothing to do in the case of a primary syphilitic sore but entirely to remove the indurated and ulcerated tissue, and the disease would, as far as the part is concerned, be at an end. Experience proves that such is very far from being the case.

When a syphilitic sore is removed by excision, as may readily be done when it is situated on the extremity of the prepuce, the cut surface will in a few days take on a specific action. This will occur, as we have witnessed, even when the greatest care is taken not to allow any of the matter from the chancre to come in contact with the cut surface. Such an action taking place in a part apparently healthy, at some little distance from the original sore, presupposes some antecedent change in the tissues in which it originates—a change produced by the infecting poison, but not capable of being appreciated so long as the diseased action had its development in the original chancre. As soon, however, as the first centre of the morbid action is removed, a similar action is commenced upon the neighbouring cut surface. The observation of such cases proves the existence of a subtle morbid process affecting the tissues in the neighbourhood of the part first affected, and necessarily producing *some* effect upon their nutrition. It appears, under these circumstances, much more in accordance with that which is known to happen in the case of other poisons, to suppose, when the constitution becomes affected with syphilis, that the disease is communicated directly to the blood circulating through the parts in which the above-mentioned morbid actions are going on, than to refer the symptoms to the passage of the poison primarily through the absorbent system.

When the constitution becomes affected in consequence of the inoculation of the vaccine or the variolous poison, an affection of the lymphatic vessels and glands certainly forms no essential part of the process. Few, indeed, have thought it necessary to invoke the aid of the absorbent system in order to account for the action of these poisons upon the animal economy, and we believe that it is equally unnecessary in the case of the poison of syphilis.

It has appeared necessary thus to enter at length upon this subject, to prepare the way for the consideration of the somewhat discordant views which we have presented to us in recent works, and especially in order that we may clearly distinguish between the entrance of morbid fluids (even if impregnated with syphilitic matter) into the lymphatic vessels, and the absorption of the syphilitic virus into the constitution.

M. Vidal, with Hunter, acknowledges four kinds of absorption of the syphilitic poison, by means of the lymphatic vessels:

1st. Where the venereal matter has been applied to a sound surface, and has produced no local effect upon the part, but has been absorbed immediately upon its application.

2nd. Where the absorption takes place from an inflamed surface. Such is the way in which a bubo is formed in cases of gonorrhœa, some of which M. Vidal maintains to be really syphilitic, although there may be no ulceration of the urethra.

3rd. Where the syphilitic poison enters the absorbent glands from an ulcerated surface; and, 4th, from a wound. All these four kinds of absorption by the lymphatics we acknowledge, but not as being capable of producing general syphilitic infection.

Independently of these means, a bubo is said, by many authors, to originate from sympathy. M. Vidal justly asks, sympathy with what? Whence does this sympathy originate? How is its influence determined to one particular part?

We must confess that this word "sympathy," as applied to the formation of buboes, has always appeared to us most unintelligible. We can readily conceive that parts which have a natural relation to each other may be affected by sympathy. Thus we can easily believe that the breasts may sympathise with the uterus, or the testicles with the urethra. But that one particular class of absorbent vessels should have a peculiar sensibility for the diseases of another part with which they have no particular connexion (independent of the transmission of their contents), it is difficult to imagine. That an uncertain amount of an unknown influence should in this unknown way be conveyed from one part of the body to another, we cannot but regard as one of the mysteries of an occult science. MM. Maisonneuve and Montanier divide syphilitic affections of the absorbent glands into those in which the glands are enlarged (*engorgement*) and those in which they are manifestly inflamed. To the latter they apply exclusively the term bubo.

> "The real bubo," it is said, "is only found with unindurated chancre, the indurated chancre, only, is on the other hand, accompanied with an indurated enlargement of the glands: from this it consequently follows that a bubo properly so called, like the chancre which precedes it, is only a local affection, and never a sign of the infection of the system If the chancre is already healed, a bubo will indicate that it was unindurated; an indurated enlargement of a gland, on the other hand, will without doubt afford evidence that an indurated chancre has existed, or is still present." (pp 161, 165.)
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> "The induration of a chancre is the certain proof of a constitutional infection It is necessarily accompanied by an indolent enlargement of the absorbent glands nearest the seat of the disease, and in a longer or shorter interval, generally within six months, is necessarily followed by constitutional symptoms, when not prevented by medical treatment." (Robert, p 211.)
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> "The bubo is of no consequence, but as a local disease; the enlarged ganglions are of the highest importance in relation to constitutional syphilis." (Maisonneuve, p. 165.)
>
> "A simple chancre accompanied by a suppurating bubo never communicates constitutional syphilis." (*Ib.* note, p. 110)

M. Vidal does not allow that the condition of the absorbent gland is of the value as a diagnostic sign which is implied above. Inflammatory bubo, he justly says, may come on independently of the absorption of any syphilitic matter, and the indolent non-inflammatory bubo may happen in consequence of strumous affections, as a result of a simple unindurated chancre, or as a secondary syphilitic affection. The observations of

Dr. Egan, again, have led him to conclusions somewhat differing from those of M. Ricord.

"Buboes," he says, "usually consequent on gonorrhœal inflammation are uninoculable, although here a mild description of secondary symptoms is occasionally met with." (p. 39.) Dr. Egan believes likewise that, after abrasions of the mucous membrane, and also after superficial non-indurated primary ulcers (whether the secretion from them is capable of being inoculated or not), a mild form of constitutional symptoms may ensue.

The opinion of the French school, represented by MM. Ricord, Maisonneuve, Montanier, Robert, &c., is, as we have seen, opposed to these views. These gentlemen find, that whenever they have had an opportunity of tracing the natural progress of the disease, an indurated chancre is always communicated by an indurated chancre, and they believe that this alone will give rise to constitutional syphilis.

The experiments and observations of Dr. Egan have led him to the conclusion that there are two syphilitic poisons. From the fact that when the matter of a phagedenic ulcer is successfully inoculated, the result is an ulcer presenting the same characters, he infers "that the virus generated by the simple primary ulcer and the phagedenic sore is as dissimilar in quality as it is in its affects." (p 51.)

The results of the experiments upon which this idea is founded we regard as manifestly inconclusive, because the inoculations were made on the patients themselves, in whom the primary affections had already assumed a phagedenic character. It is likely enough that, if in these patients one venereal sore presented a phagedenic character, a second or third would do so likewise. In order to be of any real value, the experiment must be performed upon persons who have not previously had the venereal disease, and whose constitutions are free from any unusually disturbing influences. As such experiments, for obvious reasons, cannot with propriety be performed, we are thrown back for our facts to clinical observation and to the information which may have been obtained from inoculating patients already infected. From the former we believe the point cannot be established; and from the latter it has been shown that ulcers produced from the inoculation of the discharge of simple venereal sores will sometimes become phagedenic.

> "If pus, taken from a simple non-indurated sore which has lasted a fortnight, and is accompanied by inflammatory bubo, be inoculated, a simple venereal ulcer will result, provided the inoculation be properly performed. This ulcer may become phagedenic, serpiginous, or gangrenous. But it never will produce an indurated sore." (Maisonneuve, p. 139.)

If, then, the virus from a simple sore will produce a phagedenic ulceration, as we have no doubt it will, we cannot regard the effect as depending upon any peculiarity in the poison.

In reviewing the whole of the works which have recently appeared on the subject of syphilis, we find, after all the experiments which have been made, and after all the attention that has been directed to the subject, that authors are not agreed as to what precise forms, either of primary or secondary venereal affections, are capable of affording a secretion which may be absorbed so as to infect a healthy person. They have hitherto failed to associate the external appearances of particular kinds of primary affections with any definite forms of secondary disease resulting from them as a necessary consequence. We have at present no generally recognised and well-defined mark of distinction between those diseases which are syphilitic and those which are not. Upon the subject of the absorption of the syphilitic poison in particular, it must be allowed that the accounts are sufficiently obscure: and we cannot but be struck with the absence of any sufficient physiological explanation of the real or supposed facts adduced upon the point in the works of the many able men lately published. Something more satisfactory, we believe, may be arrived at by attentively considering the earliest stages of the morbid processes which are involved in the absorption of the syphilitic poison. The great author of this mode of investigating morbid actions has prefaced his treatise on the venereal disease with the following remarks, which we offer no apology for introducing here, as we believe they have never received the amount of attention which they deserve, and have never, since Hunter's time, been applied in their full extent to the illustration of our present subject. "No two actions," says Hunter, "can take place in the same constitution, nor in the same part, at one and the same time; no two different fevers can exist in the same constitution, nor two local diseases in the same part, at the same time."

It might appear strange to any one who had not considered the subject in its physiological relations, that these ideas should occupy so prominent a position in Hunter's work on the venereal disease, and that they should be dwelt upon in this rather than in any other of his writings. We believe, nevertheless, they are the principles upon which much that is apparently obscure in relation to this disease may be explained, and that they afford a remarkable instance of that intuitive insight so peculiar to our great physiologist, by which comprehensive general ideas were appreciated in their extent and simplicity, even where their applicability to particular details might not have been traced.

For truth and clearness, the description of a primary syphilitic ulcer has not been excelled since Hunter's time. "A chancre has commonly a thickened base, and although in some sores the inflammation spreads much further, yet the specific inflammation is confined to this base." This *specific* action, in which the arteries throw out coagulable lymph, depends, according to the Hunterian nomenclature, upon *adhesive inflammation.* The action by which pus is formed is named *suppurative inflammation*, and that which removes parts the *ulcerative inflammation.* These three effects of inflammation Hunter regards as distinct actions, and therefore incapable of being produced in the same part at the same time. Now that which is peculiarly characteristic of the syphilitic action in a part, is a specific adhesive inflammation, which has no connexion at all, and indeed, according to the Hunterian doctrine, is incompatible, with the ulcerative inflammation. And as we have before shown that there is no evidence that the poison is taken into the system by absorption (through the lymphatics), we conclude that ulceration may form an accidental, but not necessary, part of that process. This peculiar form of the primary disease is the only one of an inflammatory nature which we can, in a satisfactory manner, trace as peculiarly and necessarily connected with secondary affections. This specific adhesive inflammation is for syphilis what the vaccine vesicle is for the cow-pox—at once a local manifestation that the disease has affected the tissue of the part to which it has been applied, and an evidence of its subsequent and consequent influence upon the constitution.

"It is true that we almost always find that a part affected with syphilitic induration is ulcerated upon its surface, and in the more advanced stages of the disease, that the parts which were at first indurated pass into ulceration. In the first case the adhesive and ulcerative inflammations affect different parts (although in close proximity to each other); in the second these distinct actions affect the same parts, but at different times.

An action commenced in a part will continue until the cause determining it ceases, or until it is superseded by some other more powerful action. If, therefore, the ulcerative action is set up by venereal infection, it will continue until the poison has expended its influence, or the part is attacked by mortification, or some other action of sufficient power to supersede it. Hence it follows that when the ulcerative inflammation has once attacked a part, it can never be followed by specific adhesive inflammation, unless some fresh poison be applied; and not even then, unless the action of the poison be sufficiently powerful to overcome the action already established. The same reasoning holds good with regard to suppurative inflammation. A most important distinction hence arises between those cases of venereal infection which are characterized *in their origin* by specific induration, and those which are accompanied by the ulcerative or suppurative inflammation.

The former class will, with tolerable certainty, affect the system, unless prevented by medical treatment, or the presence of some peculiarity or disease; the latter are never, we believe, followed by constitutional syphilis. Ulceration and suppuration, like mortification, destroy the vitality of the parts which they attack, although in a more gradual manner; and, as the syphilitic poison requires a living nidus for its development, it is destroyed in these actions before it becomes, in the process of growth, taken into the system.

These conclusions, based upon the Hunterian doctrines,* we venture to affirm, may be borne out by practical observation.

If a part inoculated with syphilitic virus be affected from the first with ulcerative inflammation, or if, from the first, there be a free secretion of well-formed pus, or if the parts affected mortify in the early stage of the disease, the existence of the syphilitic virus will cease with that of the parts which it has infected. The disease, as far as its specific characters are concerned, will be a local one.

From what has already been said, it may be inferred that when a part inoculated with syphilitic matter suppurates freely from the first, or is attacked with mortification, specific ulcerative inflammation does not take place; and, accordingly, practically we find that in these cases there is seldom any affection of the lymphatic system.

[illegible] is more than probable that Hunter would himself have arrived at similar conclusions, had not his mind been preoccupied by the idea derived from his physiological experiments of the lymphatics being the only absorbents.

When, on the other hand, ulcerative inflammation is early established, the absorbent glands become suddenly inflamed, and they generally suppurate, be the medical treatment what it may.

It is not intended by anything that is here stated to imply that vitiated fluids may not enter the circulation through the absorbent system, even although the morbid processes which give rise to their formation may have had their origin in the venereal disease. Well-marked cases from time to time present themselves, in which the lymphatic vessels, the absorbent glands, and even the thoracic duct, are found distended with puriform or sanguinolent fluid. Such diseased products poured into the circulation must, necessarily, have a deleterious influence upon the constitution, and must give rise, occasionally, among other symptoms, to eruptions upon the skin, which may, more or less, resemble true syphilitic affections. Such eruptions sometimes follow an inflammatory bubo, especially when it does not suppurate. They usually appear before the primary affection to which they are attributed has subsided; they generally last but for a short time, and do not recur. They subside readily of their own accord without any specific treatment. These affections, as we have said, may depend upon the absorption of inflammatory products resulting from venereal infection, but we cannot regard them as arising from the presence of the syphilitic virus itself.*

A somewhat remarkable case of this form of disease lately presented itself among the out-patients at King's College Hospital. A woman, who had previously had only a leucorrhœal discharge, applied for an ulcer upon her chin; this was circular, excavated, red, and glazed upon its surface. It gradually and slowly increased till it attained the size of a shilling. It was surrounded by a good deal of general induration The glands under the chin were, from the first, enlarged and indurated, and remained so after the sore was healed; those in the neck were not at all affected during the period of the patient's attendance. The secretion from the sore was carefully inoculated on the patient's arm. On the third day a small red point marked the seat of the inoculation; but on the fifth day this had disappeared, and no results of the inoculation could subsequently be discovered.

Before the sore had healed, an eruption appeared over the body of a reddish brown colour, and covered by thin scales. This affection we could not, from its characters alone, have distinguished from the commencement of a syphilitic eruption. It, however, disappeared in a few days, and did not return. No specific treatment was used in this case.

The syphilitic poison presents several peculiarities in its mode of absorption, upon which many of the peculiarities of the diseases to which it gives rise appear to depend.

There are three modes of absorption usually recognised.

1. When poisonous substances are applied to an internal and vascular membranous surface, or are introduced into a wound, or by friction upon the surface of the body are forced through the epidermis, so as to enter

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* This class of cases is probably included in Dr Egan's "mild form of constitutional symptoms." We may have on one side an ulcerated surface producing an inflammatory bubo, and on the other, an indurated sore producing a chronic enlargement of the glands only. The system will, under these circumstances, become infected; but the disease on the side where the suppurating bubo is, will not have contributed to such infection.

into and affect the system, they find their way directly into the blood, through the coats of the bloodvessels.

2. *The chyle formed during the digestion of the food is taken up from the mucous surface of the intestines by the lacteals.*

3. When the molecular structure of the body is absorbed* in the removal of parts which are not at the same time replaced, as happens in ulceration, the lymphatics are the agents employed.†

From what has been said, it will appear that the syphilitic poison is absorbent in none of these ways, and therefore that the usual laws which apply to poisons absorbed by any of these means do not necessarily apply to syphilis.

From experiments which have now been far too often repeated, it is proved beyond a doubt that the syphilitic poison may remain in contact with an abraded mucous membrane, or be inserted beneath the cuticle, and allowed to remain for two, three, or four days, and no absorption will take place. If, at the expiration of this time, an action is set up which is incompatible with specific adhesive inflammation,—if, for instance, the part is made to slough by the application of caustic,—no effects of the poison will anywhere be perceived. From this it is evident that a certain time must elapse (during which the poison enters into a kind of combination with the part to which it is applied, and produces in it a specific action) before any absorption can take place. This requisite period of incubation it is that secures the system against infection in cases where from the first ulcerative or suppurative inflammation has taken place. A part in the course of being contaminated becomes by these processes dissolved or removed before the act of absorption can be completed. Fresh parts may continue to be attacked, but these, in their turn, are destroyed before they can act as the channels of infection to the constitution. Hence arise often extensive local intractable ulcerations, which are not followed by any secondary symptoms.

It may be asked how it is, if a primary ulcer can produce a bubo which may be proved by inoculation to be syphilitic, that the poison is not absorbed from this fresh source? The answer to this is implied by what has gone before. The inflammation excited in the bubo will be, in all probability, of the suppurative or ulcerative kind, and these actions, as we have seen, are not compatible with the absorption into the system of the syphilitic poison from the parts in which they occur. We may add to this, that experience proves that every time that a fresh inoculation takes place in the same individual, and from the same original source, the effects of the poison will show themselves with less severity. We have here the secret of the absence of constitutional results from the numerous experiments on syphilization alluded to in the first part of this article. The inoculations, however often repeated, produce directly suppuration or ulceration, and consequently are not followed by any general consequences. When a gland is affected with syphilitic ulceration or suppuration, the conditions are somewhat similar to those under which a patient is placed by syphilitic inoculation. Many small spots of syphilitic ulceration and one *large spot, would, we conceive, be nearly equivalent to each other. In neither case is the poison absorbed.* This circumstance it is that has given some of our continental brethren the ideas—1st, That syphilization is a protection against syphilis; and, 2ndly, That each person can only have constitutional syphilis once during his lifetime. It is, we believe, undoubtedly true that during the time that a syphilitic sore is undergoing ulceration or suppuration, any fresh sore which may be acquired will immediately do the same, and we believe it possible that this tendency may be kept up, by repeated inoculation, for a considerable time, during which no fresh absorption of syphilitic poison will take place. The same immunity, we believe, would be afforded to the system by an ulcerating or suppurating bubo, during its continuance, and perhaps for a considerable time after it had healed. But in either case, allow an interval to elapse, during which this tendency to ulceration or to suppuration shall have worn itself out, and the system will again become subject to genuine syphilitic infection, and be again liable to fresh forms of secondary disease. We confess that we are surprised at the confidence with which some of the French surgeons speak in reference to the occurrence of constitutional syphilis once only during the lifetime of each individual.

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* We have noticed one form of bubo which appears to depend in an especial manner upon molecular absorption, independent of ulceration. After an unindurated sore has healed, as the general surrounding thickening disappears, one or more glands will become greatly enlarged. They will not suppurate nor be followed by any constitutional symptoms. This we have observed particularly after ulcers produced by inoculation.

† Mayo's Physiology.

> "For a long time," say MM. Maisonneuve et Montanier, "we have sought in vain for a person who has at two different times had an indurated chancre, and we have not yet found such a case. Lately M. Diday, the senior surgeon of the *Antiquaille de Lyon*, defied any one to produce a well-authenticated case of a person having twice had constitutional syphilis; and we believe that no one has hitherto responded to his challenge. The law is easily understood. It is the same for all diatheses. Constitutional syphilis is a disease under which the patient either recovers, or under the influence of which he remains for the natural period of his life. Once cured, he has acquired an immunity from the disease for the rest of his life. Is it not the same with the small-pox, the cow-pox, &c.? We are, however, open to admit, although no cases have hitherto been recorded in the annals of science, that as a patient might have the small-pox twice, as an exception to the general rule, so might he twice have constitutional syphilis.
>
> "This by no means proves that a person having had an indurated chancre may not contract other sores, or may not be inoculated an indefinite number of times, only that these sores will not become indurated, and consequently will not give rise to constitutional symptoms." (pp. 38, 39.)

Let us test these principles, first by what these authors themselves say in other places, and then by observation. At page 15, in a passage quoted at length in the former part of this article, these gentlemen affirm, in describing the results of the inoculation of pus from a chancre, that, "after the fifth day the subjacent tissues which before had undergone no change, or were only slightly œdematous, now become *infiltrated* and *hardened* by the effusion of plastic lymph, which to the touch gives the sensation and resistance of certain forms of cartilage." Now, as these surgeons profess to inoculate only those who are already the subjects of the disease, and affirm that a medical man is never justified in performing the experiment under any other circumstances whatever, (note, p. 139,) we naturally ask, Whence did these gentlemen gain their knowledge? By their own showing, the assertion made in one part of their work, that a patient could only have one indurated chancre, is contradicted in another, when it is said that inoculation will give rise to a second.

Experience likewise fails to confirm the views in question. Cases every now and then present themselves, in which, after patients have had constitutional syphilis, they contract indurated chancres. We have at present under treatment a gentleman, who returned to England after an absence of two years, having, before his departure, had a syphilitic sore, followed by an eruption, the stains of which were evident upon his return. He contracted fresh disease during his stay in London. Two well-formed circular and indurated chancres presented themselves on the glans penis. These were followed, in a few weeks, by a well marked crop of syphilitic lepra, of a bright copper colour, and quite distinct in appearance to the dark-brown stains of the first eruption. Similar cases might easily be multiplied, but we conceive this to be unnecessary. In such instances, one of two things must happen: either the sore which first produced the disease must have been unindurated, or else the patient must have had two indurated sores. Both these events are considered impossible, under ordinary circumstances, by the authors whom we have quoted.

We conclude, from all the observations which have been made, that a person may have an infecting syphilitic sore at any period of his life, whether he has had a similar affection before, or not; but that during the time that the system is under the influence of the syphilitic poison, the sores produced by any fresh infection will have a tendency to pass directly into suppuration, or ulceration, or mortification (including phagedæna); that the disposition to any one of these actions may be continued in the system after all other traces of the poison by which the system was first affected have ceased; that as long as this disposition lasts, the system is comparatively secure from any fresh infection, because these actions are incompatible with that by which the poison is absorbed.

It must, however, be borne in mind, in reference to these processes, that any one of them may succeed the specific adhesive inflammation, and then it will not, of course, prevent the infection of the system, which may already have taken place; nor will the character of the disease then afford evidence of its having been thus infectious.

The specific adhesive inflammation may continue a very short time; it may be of a very limited extent, sometimes not exceeding in depth the thickness of a layer of parchment; or it may be masked by surrounding general infiltration. These accidental peculiarities will not prevent the entrance of the poison into the system, but the circumstances attending them will influence the form under which its secondary effects appear.

The existence of non-inflammatory modes of contagion, such as the absorption of the syphilitic virus without any change of structure in the part to which it is applied, or the transmission of constitutional syphilis by means of mucous tubercles, or excrescences not having an inflammatory origin, forms a subject for separate consideration.

Henry Lee.

REVIEW X.

1. *Homœopathy: its Tenets and Tendencies.* By J. Y. SIMPSON. Third Edition. 8vo.—*Edinburgh; London,* 1853.
2. *The Sophistry of Empiricism.* 8vo.—*London,* 1853.

IN religious or moral inquiries we have definite courts of appeal for the settlement of doubtful points, and for testing the accuracy of views, statements, or opinions. We appeal either "to the law and to the testimony," (and "if they speak not according to these, it is because there is no truth in them,") or to our own conscience, which either "excuseth or accuseth." It is true that different interpretations of meaning may be given by different apparently equally educated men, to the same *dictum;* but, *cæteris paribus,* they agree so much in the main, that sufficiency for rule of conduct and life is attainable from these sources, even by the "wayfaring man, though a fool." Religion and morality are psychically essential in society, as food and raiment are physically; hence they are more or less practically or theoretically viewed by all thinking men. Science, in the ordinary sense of the word, is not essential; and the so-called "departments" of science appeal with interest only to certain definite classes, which classes, even in combination, form quite the minority of mankind. We shall presently see that this is a necessary condition of society, and that science grows slowly, and almost invisibly; that it is as a shut book to the majority, and that a thorough appreciation of any one even elementary subject, may demand a considerable acquaintance with several departments of science. For example: who could understand astronomy, without the knowledge of mathematics? or physiology, without the assistance of chemistry, botany, and anatomy? And each one of these subjects has, in like manner, its dependencies. So that, really, there is nothing like isolation in any department of science. Its various subjects are distinct for a time, like the springs and streamlets of a mighty river, but like them converging to its course, and intermingling their particles in its onward flow to the ocean, which ocean again gives forth its clouds and mists and rains, again, in their turn, to become springs, streamlets, rivers, and oceans. And yet we find men, with a temerity equalled only by their ignorance, asserting without evidence—believing without facts—experimenting without observation—manufacturing sciences, forsooth, out of clever-sounding words and showy tricks; and palming them off—upon whom? My most credulous public—the million; and the million likes it—and pays for it—and flatters itself that it is scientific—and congratulates itself on the "progress of knowledge," and buys a guinea's worth of ready-made science; or goes and hears a lecture on mesmerism, phrenology, globulism; or purchases a book on hydropathy, table-moving, or ghost-seeing, or spiritual rappings. And others like it—"*media*" like it—we mean the discoverers of these sciences. Of course they like it—they have every reason to like it. It is Australia and nuggets, without the journey or digging, to them—with the reputation of being the founders of *new sciences,* for so the million designate these delusions.

In religion and morality they have the best guides—but in science, nothing beyond common sense, which, though they may possess it, they cannot always use it, for they have not learned "the trick of answer." Nothing, however, is so essential to them as a knowledge of the sources of fallacy in scientific observation. Now, in reviewing these works and systems of popular delusion, we shall *first* examine the natural progress of scientific truth, and, *secondly*, give certain popular tests of scientific accuracy, which may form a "law and testimony" to those who have neither the time, nor inclination, nor "knack," to think out *tests* for themselves.

A knowledge of the sources of error is the truth-seeker's best safeguard; for error is everywhere, oftentimes concealing the truth utterly, and oftentimes so simulating its qualities, that it is mistaken for it. Before we decide upon the question of the right or wrong of anything, we must first settle what is the *kind* of inquiry we are about to institute. Is it to determine the error of truthfulness, or the truthfulness of error?—and even this decision may be a source of fallacy, for our judgment may be warped, or our reasoning illogical. We verily believe that there are few errors so erroneous, or few fallacies so fallacious, but that they contain in their history some gleam, if not a remnant, of truth; and as error is the negation of truth, or its absence, though a quality positive in its action, so there is no truth so pure, so brilliant, or so absolute, but that it may be sullied, dimmed, or shaken by error—not ultimately so, for truth is eternal, but for a time; for error, though of short duration, is like "sorry weeds of rapid growth."

We are about to speak of pseudo[*] science. Now, what is SCIENCE?

The derivation of the term partially answers the question—("*scientia*"[†]) KNOWLEDGE; but its conventional usage implies something more—"knowledge systematized," "knowledge arranged in departments," &c. The imperfection of our acquaintance with the secrets of nature, and of our capacity for the absolute acquisition of *all*[‡] knowledge, forces upon us the necessity of division and subdivision of science, which is *one*, a perfect entity, into parts and parcels.

To investigate the details of these particular divisions, to group them under general principles, to view their co-relation with other general principles in other divisions of this systematized knowledge, so as to trace the phenomena of the whole range of the so-called sciences, physical and metaphysical, to their *ultimate facts*,—is the highest ambition and the most steady aim of the natural philosopher.

Imbued with a perfect love of truth-seeking, he commences this inquiry acknowledging two great principles:

First, that there *are* ultimate facts in nature—points beyond which he cannot go, bounds to his finite reason.

Secondly, that science is perfect in proportion to the *paucity* of its ultimate facts.

Facts which are ultimate now, may have a resolution in some more distant law as yet unknown. The full appreciation of this feeling, therefore, is a difficult attainment, and humiliating to a degree; elaborate processes of logical induction, yielding magnificent results in any particular inquiry, have to be forgotten; and the results themselves, which seemed the very goal of a most worthy ambition, serve only as the starting point for a new view, like the apparent limit of a vanishing perspective.

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[*] "Ψευδής, false, lying, deceiving."

[†] Scientia was used in a subjective sense by the Romans, not in the objective, like "*doctrina*," or "*disciplina*."

[‡] In connexion with this idea, the reader might consult the views of Bishop Butler in his fifteenth sermon, "Upon the Ignorance of Man," with great advantage.

Philosophers are not, like poets, born philosophers—they have to make themselves so; they are instructed by that great schoolmaster, Observation, and they educate themselves by reflection and judgment. The reason that we can learn better than our ancestors—that is, more in the same time—is not that there is proportionably less to learn, but that everything is more systematized, and hence appears less, because it is not scattered. Our "helps to read," and think, and experiment, are better—and posterity's will be better than ours, if we do our duty. In this world, this school of science, we find nothing ready made, save the implements and subject of study; but we do find better tools and better appliances than our ancestors possessed. 'Tis nothing that you boastingly point to Optics, Chemistry, Physiology, Mechanics, and say, See how these have suddenly enlarged!—how improved—how perfected! All this is true. But why is this? It is not contrary to the law that science is necessarily of slow growth, for experience and the evidence of our senses teach the reverse; the reason of these rapid strides is, that after ages of incubation, the fulness of time has arrived in their special histories. The April shower descends precipitately and unwarningly from a sky a moment ago blue and serenely beautiful, but its cloud has been long forming, from the accumulated myriads of invisible vesicles of vapour. A coat of mail is knitted and jointed in a few hours, of which the numberless links each take as long to form. And so with these apparently rapid growths of science; they are not the evolution of one mind, of one moment, or of one generation, but the developments of patient study, of quiet speculation, of steady and repeated experiment. Convinced of this, every scientific man looks with doubt, great doubt, on ready-made sciences. That, mushroom-like, they should spring up in a night, is, at least, a most suspicious feature in their character, if not an absolute *test* of their fallacy. Wisely he suspends his judgment, if he be not prepared to deny their claim to truth, until, by thought inductive and deductive, with experiment, he have tested their accuracy. Then he boldly denies their premises of argument; or admitting these, their modes of reasoning; or admitting both, their powers or qualities of observation. And how is he met? He is referred to Galileo, and told ~~of his~~ sufferings for truth, or to Harvey and his persecution. He is told that the "blood of the martyrs is the seed of the church," and all such illogical effrontery—effrontery as far as they apply to him; for such assertions are the very *petitio principii* in dispute. Admit them martyrs—Galileos or Harveys—and the question is settled: they are right, and you are wrong; they are persecuted, and you are a tyrant. But mark the difference. How did Galileo arrive at his conclusions, and what use did he make of them? Where is the analogy? . . . How absurd this line of argument!—they require of you to believe in a dogma of their own creation, because some one did not believe in certain dogmas not of their own creation, and were wrong; and they never, of course, show you the analogy between the cases, either in doctrine, discoveries, processes, or results. That is to say, *somebody did not believe something which turned out to be correct, therefore you should believe everything, lest you should fall into the same error!*

If this line of argument lead to anything, it at once tends to stop all inquiry into the truth of assertion or experiment, and demands a tacit belief upon the *ipse dixit* of anybody. Not that we would have the philosopher or scholar troubling his mind with all the trifling trash of the petty inquiries instituted by these pseudo-sciences; he should have higher aims. With a few commonplace tests, they are precipitated from all position, and can hold no dominion in his judgment. Thus far, then, we have given one or two tests or hints at them, by which the "spirits may be tried." But before applying them exactly, let us see how the *real* philosopher proceeds in *his* inquiries. Things appear strong by contrast.

Unprejudiced by preconceptions, his search is after truth—for truth's sake. He is not bent from the even tenour of his way by difficulties, dangers, or disappointments. He loses not the means of success by solely contemplating the goal of his ambition. His life is action, and he is satisfied even with no positive results to an inquiry instituted, provided he be sure the inquiry was logically reasoned. *Negative* results are to him frequently as satisfactory as the most positive sequences, and he would rather give up an hypothesis upon which rested a most beautiful and plausible theory—yes, after cherishing it most dearly—than sell his mind to deception, or give himself up to "believe a lie." He does not jump at conclusions, reasoning from particular instances to general principles. He does not deal in assertions whose accuracy he cannot demonstrate or logically prove. He is never tired of accumulation of evidence, nor in an investigation does he cry out at any moment, until he have conquered the difficulty, "Hold, enough!" Quietly, steadily, patiently, noiselessly he works—by the midnight lamp or in the laboratory, the dissecting room or the studio, impatient only of interruption. He wants not the plaudits of an admiring audience, nor an advertisement in the *Times*, to tell of his discoveries. Your real discoverer is generally too captious of error to court scrutiny till "time and place shall serve," and then too modest to receive praise. Yes, the mental satisfaction of knowledge acquired is too ethereal an exaltation to be approached by the sordid enjoyment which comes from the flattery of others.

But let us now proceed to examine the sources of error in scientific inquiries.

There are twelve very common sources of error, which, if known, may serve as TESTS in the investigation of the accuracy of any subjects, which, either from their intrinsic merit or the weight of authority, are presented to the acceptance of our belief:

I. Errors arising from mistakes about the meanings of terms.
II. Errors arising from the substitution of names for things.
III. Errors arising from the substitution of assertions for facts.
IV. Errors arising from illogical reasoning from correct data.
V. Errors arising from illogical reasoning from incorrect data.
VI. Errors arising from logical reasoning from incorrect data.
VII. Errors arising from partial instead of complete observation.

VIII. Errors arising from mistakes in observation.
IX. Errors arising from incapacity for observation.
X. Errors arising from the innate love of the marvellous.
XI. Errors arising from the system of discipleship.
XII. Errors arising from the system of leadership.

We will now consider these *seriatim*, illustrating what we conceive to be fallacies from various works on systems of popular delusion, whose names, however, do not appear at the head of this article, simply because we would not give them the individual importance that an appearance in a really scientific journal would demand for them.

I. ERRORS ARISING FROM MISTAKES ABOUT THE MEANINGS OF TERMS.

Mistakes arising from a different appreciation of the meanings of terms, is one of the most common sources of error—one, too, into which even the conscientious and scientific may fall without being aware of it. Hence, it is of the highest importance to define clearly all words used in argument or dispute before commencing any set of inquiries. Very often the dispute is ended by such a definition, so that those a moment ago antagonistic and ready for severe strife, find they are of the same opinion on the point in hand. Words are dangerous weapons, they require careful but tight grasping, and when used either in attack or defence should be so equally matched and presented, that, as in a "fair pass of foils," it matters not which we or our antagonist may select, both having the same appreciation of the same weapon.

There are certain words constantly occurring in scientific and pseudo-scientific writings, which have become as it were stereotyped, and which, if they have not by usage been utterly worn from their original impression and sharp outline, still want "biting up." Some words become obsolete for want of usage, but more become obsolete in their exactest signification from being too frequently used, or rather *ab*used; they have palled upon the sense as does monotony on the ear. Take a set of hackneyed words, such as "cause," "effect," "phenomenon," "inquiry," "hypothesis," "theory," "fact," "experiment," "law."

Now, to define these words *here* is not our task—we will reserve this for a future occasion and another place. We merely wish to state, that these words are "suspicious characters,"—we must keep an eye on them—watch them with carefullest scrutiny. Remember that an INQUIRY is not merely asking a question—that EXPERIMENT is not merely *doing* something—that HYPOTHESIS is not merely the Greek for the Latin "supposition," or the English "guessing"—that THEORY is not merely something visionary, or that which is not practical—that PHENOMENA are not merely *wonderful* appearances, or necessarily so—that the word FACTS among such terms is most of all suspicious—very dangerous indeed. We must look very carefully into every sentence which begins thus—"It is a well-known fact"—the more especially if that sentence have a "therefore" in it, or "consequently," or "so that," or any notion of a sequence.

We must remember how we use the terms CAUSE, EFFECT, LAW—that which we call "cause" is at best only an effect of a great cause, and some so-called causes are probably only minor effects of some major cause unknown to us; that effects are resultants of our admitted causes, and that they are as dependent upon one another as rivers and seas. And we must remember, lastly, that to know the meaning of the term LAW in scientific language, requires considerable instruction and much more education.

So much, then, for the importance of attending to the meanings of words, and the necessity for the utmost exactness of definition in scientific argument. The abuse of language arising first from its own imperfections as a vehicle of thought; secondly, from *our* want of exactness and skill; and, thirdly, from our *opponents'* non-appreciation of our positive meaning—is a most common and fruitful source of error. It extends to sentences, phrases, formulæ, and can be guarded against only by rigid discipline and conscientious motive.

II. ERRORS ARISING FROM THE SUBSTITUTION OF NAMES FOR THINGS.

"What's in a name? that which we call a rose by any other name would smell as sweet." Of course it would to Juliet, because she was aware, in the case in hand, that the qualities of "the thing" were not altered by this substitution; nor would it be of the slightest moment if we were, with Juliet, in all the cases to which we refer, equally informed. But this is not so, and it is impossible it could be so at all times. From several works before us upon "Spiritual Rappings," "Clairvoyance," "Mesmerism," &c., we can find numerous examples; let us analyse the first, namely, "Spiritual Rappings." The *thing* in question, that is, admitting it to be something, is a manifestation from the spirit-world to this natural world, and through our material bodies to our immaterial minds. The book says, "The facts are these" (reader, beware of "facts")—"the medium sits or stands in a room near a table, the inquirer asks, 'Are there any spirits present?' whereupon a tap or flutter is heard in affirmatory answer; 'Will the spirit of ——— appear?' is next inquired, and sometimes the request is granted and ——— spirit appears! Any question put by the inquirer through the medium is then answered, either in relation to the past, the present, or the future." Without proceeding further, let us observe, that sound is a physical phenomenon produced by the forcible meeting of two material substances, and thereby causing such a set of undulations in the atmosphere as shall, by striking upon the tympanum of the ear, give that sensation to which we apply the term "hearing." Now, if these sounds be physical, as all sounds must be, they are necessarily not spiritual, for spirit is immaterial, and according to this definition, can produce spiritual or immaterial effects only, unless, as in its conjunction with an organism or body, it have a material agent and suitable apparatus. "Rappers" do not point out any such agency, nor show any organism; nay, they boldly assert, and get the spirits to endorse their statements, that spirit, and spirit only, produces this sound. But we as boldly affirm, and that upon experience and experiment, that if there be "rappings" they must be physical, and if physical they cannot be spiritual. Now, at first, it would appear that this subject scarcely comes under this division, inasmuch as we have shown that it is the substitution of a name for *nothing;* yet, it must be remembered that what we call "nothing," and believe to be nothing but gross deception, these "rappers" vaunt as equal in integrity to the prophecies of old, or the direct revelations of God to man, and then impudently attach it to the physical sciences as a method of physical research.

The worst effect of a nomenclature is, that the moment we have given anything a name, it more or less stops inquiry into its history and nature, especially if the name have a popular signification. In cases like the present, where a positive name is given absolutely to nothing, it gives that nothing an existence in the minds of the uninitiated, and thus far has a creative power, so to speak, that is, makes something out of nothing; but in other cases where a name is given, which more or less defines some qualities of the thing described, the thing having a real existence, this naming too often stops inquiries, as in the other case, and we substitute it for the qualities of things, that is, if the thing be named properly some of the qualities are substituted for all the qualities, or the mind rests satisfied with some while we might know many more. This is remarkably the case in all partially developed subjects, and is at the same time a cause of that partial development. In therapeutics, for example, opium has been called a "narcotic;" many persons whenever they want a narcotic give opium, not as opium, but as a narcotic, and only a few look into the thing further and perceive that opium is something more, and is a compound of many diverse and extraordinary substances of different effects—and so we might say of almost every vegetable drug in the Pharmacopœia. Now, what is the result of all this? why, our therapeutics consist in great measure of a system of names, and how could we expect otherwise?—names are prescribed, that is are substituted, for things, and of course you can get names only as the result.

Thus, this source of error is not confined to the pseudo-sciences, but certainly is most richly developed in them, as might be expected, and in real sciences is found, as before hinted, in those principally which from partial development have fewest absolute facts.

A balloon ascends, a stone falls, and if we ask any semi-scientific man the cause of these apparently diverse phenomena, he considers that he has acquitted himself as a scholar, if not something more, when he answers flippantly, with a supercilious pitying of your ignorance, "gravitation;" we are, however, not satisfied, although we believe in "gravitation," and while we leave this scholar to tell us *what* gravitation is, and how much there is of the substitution of a name for things in *his* use of the term, we will go on to the next head—viz.

III. ERRORS ARISING FROM THE SUBSTITUTION OF ASSERTIONS FOR FACTS.

We know the value of "on dit" in relation to all inquiries, and most of us look suspiciously at any statement backed only by "they say so," and yet many will readily believe anything which appears in print—"it must be true," as is a common expression, "because I read it in a newspaper," or in a book, forgetting what Burns has sung—

"Some books are lies frae end to end,"

and if we might pay attention to the *traditional* pseudo sciences we might continue—

"And some great lies were never penned."

Now we have before us a book of the class referred to, "Table-Moving, by a Physician," a most appropriate name, effect, and cause, neatly combined! for when the table did move it was most likely by the assistance of the physician. But Professor Faraday's experiments and writings in connexion with this subject so thoroughly settled the question, that we need do no more than quote part of them; referring the reader for further information to the 'Athenæum' for Saturday, July 2nd, page 801:

"The proof which I sought for, and the method followed in the inquiry, were precisely of the same nature as those which I should adopt in any other physical investigation. The parties with whom I have worked were very honourable, very clear in their intentions, successful table-movers, very desirous of succeeding in establishing the existence of a peculiar power, thoroughly candid, and very effectual. It is with me a clear point that the table moves when the parties, though they strongly wish it, do not intend, and do not believe that they move it by ordinary mechanical power. They say, the table draws their hands; that it moves first, and they have to follow it,—that sometimes it even moves from under their hands. With some the table will move to the right or left, according as they wish or will it,—with others the direction of the first motion is uncertain;—but all agree that the table moves the hands, and not the hands the table. Though I believe the parties do not intend to move the table, but obtain the result by a *quasi* involuntary action,—still I had no doubt of the influence of expectation upon their minds, and through that upon the success or failure of their efforts. The first point, therefore, was, to remove all objections due to expectation, having relation to the substances which I might desire to use.—so, plates of the most different bodies, electrically speaking—namely, sand-paper, millboard, glue, glass, moist clay, tinfoil, cardboard, gutta percha, vulcanized rubber, wood, &c.—were made into a bundle and placed on a table under the hands of a turner. The table turned. Other bundles of other plates were submitted to different persons at other times,—and the tables turned. Henceforth, therefore, these substances may be used in the construction of apparatus. Neither during their use nor at other times could the slightest trace of electrical or magnetic effects be obtained. At the same trials it was readily ascertained that one person could produce the effect; and that the motion was not necessarily circular, but might be in a straight line. No form of experiment or mode of observation that I could devise gave me the slightest indication of any peculiar natural force. No attractions, or repulsions, or signs of tangential power, appeared,—nor anything which could be referred to other than the mere mechanical pressure exerted inadvertently by the turner. I therefore proceeded to analyze this pressure, or that part of it exerted in a horizontal direction—doing so, in the first instance, unawares to the party. A soft cement, consisting of wax and turpentine, or wax and pomatum, was prepared. Four or five pieces of smooth, slippery cardboard were attached one over the other by little pellets of the cement, and the lower of these to a piece of sand-paper resting on the table; the edges of these sheets overlapped slightly, and on the under surface a pencil line was drawn over the laps so as to indicate position. The upper cardboard was larger than the rest, so as to cover the whole from sight. Then, the table-turner placed the hands upon the upper card,—and we waited for the result. Now, the cement was strong enough to offer considerable resistance to mechanical motion, and also to retain the cards in any new position which they might acquire, and yet weak enough to give way slowly to a continued force. When at last the tables, cards, and hands all moved to the left together, and so a true result was obtained, I took up the pack. On examination, it was easy to see, by the displacement of the parts of the line, that the hand had moved further than the table, and that the latter had lagged behind;—that the hand, in fact, had pushed the upper card to the left, and that the under cards and the table had followed and been dragged by it. In other similar cases when the table had not moved, still the upper card was found to have moved, showing that the hand had carried it in the expected direction."

This simple experiment of Professor Faraday's is a beautiful illustration of the true method of scientific inquiry. Not that for one moment we would suggest that the importance of a scientific phenomenon was sought for in these "table-movings," or that the attention of a scientific philosopher called to the investigation of any subject necessarily confers upon that subject any of the importance and moment of his ordinary pursuits; but this philosopher's mode of investigation is a happy contrast to the ready belief of the partially informed and those of the class to which we have previously referred, who quote Galileo, Harvey, and the Christian martyrs, as proofs of subjects which bear no analogy to their references.

IV. ERRORS ARISING FROM ILLOGICAL REASONING FROM CORRECT DATA.

This is not so common as many other sources of error, and frequently proceeds from carelessness rather than from incapacity; we might almost say invariably so, if the data or premises of the argument have been discovered or observed by the reasoner for himself. For correct observation involves so many exact and careful qualities of mind, and so much common sense, with at the same time no little mental calibre, that any one having once exercised these qualities on any particular point, would almost necessarily have power enough to conduct a process of correct inference towards a judgment or conclusion thereon; for observation, let it be remembered, is not merely looking at a thing. "The tables move," says a learned author in a work before us, "I have many times seen them. I know there is no wish to deceive on the part of the manipulators; moreover, the moment you take your hands away the movement ceases, *therefore* (the italics are our own) it *must* be from some magnetic or similar influence exerted by the manipulators upon the table." Therefore? there is no "therefore" in the case; it is no more a consequence of any expressed premise in this argument than if the conclusion referred to the price of corn in the year 1828, or the probable cycle of the next planet to be discovered. A table moves, you see it, you are convinced that no intentional deception is practised or attempted, the movement ceases when the hands are removed, *these are your expressed premises,* and so far, all is correct. Now, according to experience and ordinary observation, the most legitimate conclusion is, that the hands as physically moved the table as if they had intended to do so; and further, according to experience and ordinary observation on "magnetic and similar influences," *im*position of the magnetizing body is not required to produce the so-called similar effects, but simply its proximity; and further, the so-called similar effects are not similar but different: and in short there is nothing but dissimilarity observed throughout any *exact* comparison of these phenomena, and table-movings would be as likely to remind any one who was really acquainted with magnetic influences of those influences, as they would of the revolution of our earth, or "Belshazzar's feast," or anything else *not* connected with the subject.

We were going up Oxford-street immediately after writing the few foregoing sentences, when we observed that the greater part of a frontage was altering. None of the men were at work at the time; but one man stood with the palm of his hand against that part of the wall which had not been taken down; it seemed to move, and when he took off his hand the motion ceased. We are convinced this man had no intention to deceive any one in the matter; nevertheless, on reapplying his hand the wall fell down, much to his astonishment and our own, but we certainly did not come to the conclusion that this was the result of "magnetic or similar influences;" nay, rather that the premises and foundation of this wall were like the premises and foundation of our friend's argument, too weak for its support.*

Now we will tell this gentleman how he might have given us a little more trouble in the argument—simply by taking a little more himself, and by adding one *datum* or premise more to its support—namely, this—"And I further know that none of the experimenters exerted any physical force in the trial." Although we should think this a rash statement, we would nevertheless endeavour to meet it, because we believe the gentleman to be conscientious; but we should remove the consideration to the next head, namely, illogical reasoning from incorrect data.

V. ERRORS ARISING FROM ILLOGICAL REASONING FROM INCORRECT DATA.

Illogisms of this kind are necessarily most fraught with error; they are so apart from truth that we verily believe it is the boldness with which they are uttered and promulgated only which demands for them any credence—wrong in foundation—wrong in process—wrong in result. We will take an example from that admirable work of Professor Simpson, on "Homœopathy, its Tenets, and Tendencies, Theoretical, Theological, and Therapeutical," third edition. It would seem as if Dr. Simpson had taken for his motto in writing this book, "*Out of thine own mouth will I judge thee*," for such a collection of contradictory assertions—such a mass of confused and trifling reasoning—such an absence of all legitimate conclusions from the data reasoned upon—were never perhaps presented to the public, except in the writings in which these select specimens were originally presented to the world. The book consists of twenty-one chapters and an appendix, and will well repay any one's perusal even as a source of amusement. We should especially recommend Dr. Simpson's happy quotations to professors of logic, as a kind of text-book of "examples in illogisms," or exercises for students upon the logical classifications of various errors.* Does nature cure by similars? "*Similia similibus curantur*," answers Hahnemann. This motto at the real basis of homœopathy is crude, quaint, and false, (and not even the best Latin in the world; we should have preferred "*medentur*," though, unfortunately for Hahnemann, that would not have altered his facts or given them a show of truth.)

In the first place, "like diseases" do not "cure like diseases;" secondly, if they did even in some instances, which they do not, *all* like diseases do not cure all like diseases, and this is the tacit *petitio principii*. Now this is important, though generally overlooked, for the error in this respect is a good instance of Hahnemann's style of reasoning. He thought he found out this grand secret from the observation of the effect of quinine in ague and in health, and forthwith from this special instance reasoned to the general law, "*similia similibus curantur.*" The observation, even if correct, could be suggestive only of an *hypothesis;* the evolution of the *law* was illogical. Now let us take another vaunted proof of homœopathic curing—"Your hand is burned, and the best cure is to hold it to the fire." Now those who with any pretensions to surgical skill make such a statement are either ignorant or dishonest, or both. First let us look at the case; surgeons make a very important distinction between different burns, founded upon the degree of their intensity; they are called of the first, second, or third degree, and it is to the first two varieties only that this plan of treatment refers, and therefore not to "burns," but to *some* burns. But after all it is not a *plan* of treatment, but merely a temporary resort, and that not according to the law *similia similibus curantur*, in which case the attempt should be to *simulate the burning*, that is, to burn again; but a nurse applies the heat temporarily *to keep away the cold air*, and not to simulate the burning. This, however, is just the style of illogical reasoning from incorrect data which pervades the whole system of homœopathy.

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* Page 151.

VI. ERRORS ARISING FROM LOGICAL REASONING FROM INCORRECT DATA.

These are errors more common among scholars and scientific men than almost anywhere else. The qualities for the observation of facts or phenomena may be perfect, but unexercised; the things stated unobserved or unexamined; and processes of reasoning only gone through, rather than a thorough sifting, not only of arguments, but the grounds of the argument. There is a very striking exemplification of this in the history of one of the most advancing sciences of the day—chemistry. Till a comparatively few years ago, it was stated that there were four *elements*, and that these four elements were, earth, air, fire, and water. The chemical definition of an element was then as perfect as now, but we have now very different notions both of the number and nature of the elements. Given, these four elements, and all material substances must be made up of them—either by a direct or indirect union—the differences in the substances produced being the result either of the nature of the union, the quantity of the material, or the method of commixture. This is necessarily a logical and legitimate conclusion from these data, but the data are false; for example, EARTH is made up of between fifty and sixty elements, and is thus far itself a compound, both chemically and mechanically. AIR is composed of all substances capable of existing in the gaseous state at the ordinary temperatures, and essentially of one part of oxygen mixed or diluted with four parts of nitrogen. FIRE is no element at all, but the resulting phenomenon of chemical combination attended with heat and light. And WATER, finally, is a compound easily separable into its parts. Now, the reasoning was logical enough beforehand, but the facts, it will be observed, were wrong, and therefore the conclusions were false.

Now, the pseudo-science, phrenology, in many particulars, and specially in its *stand-punkt,* or foundation argument, is equally erroneous. It is conceded that the brain is the instrument, or, if you will, the *seat* of the mind—so far all physiologists allow; but phrenologists state further, that the brain has organs, or special developments, which correspond with certain qualities of mind, intellectual, moral, and animal; and that these are marked by certain prominences and depressions upon the cranium; that therefore these being known, the character of the individual may be described. Now, if these data were correct, the conclusion would be true, for the reasoning would be logical; but, unfortunately for phrenology, in the first place the brain itself has no such organs, and they have, therefore, never been demonstrated: in the second place, there is no *necessary* correspondence between the surface of the brain and the *external* conformation of the skull: and, thirdly and lastly, the *suppressed premises* in this phrenological argument are also incorrect, or subvert the argument itself —for, allowing there to be organs in different situations upon the brain, when any combination of them is called into exercise, *what is it which communicates between the one and the other?* How does *it know* how to select the right organ, and when? *And if it do know all this,* what need has it of these organs at all? for surely there is mind enough in this very selective power for all the effects we ever see produced by mind—the mind must have all the feelings, capacities, perceptions, &c., without the organs, or it would not know one organ from another; and if it have all these things, they are not the cause of variety in mind, therefore they can be no guide to that variety; and they are not the effect of variety in mind, for they are developed in childhood, in absolute infancy, nay, in fœtal life, ere yet the mind can exercise variety; for, say the phrenologists, the cranial developments are the result of a moulding process of the brain and its organs upon the bone. Now, either the brain moulds the shape of the bone, or the bone moulds the shape of the brain. The phrenologists must believe this (though we do not), for they assert that the one corresponds with the other, otherwise, indeed, their developments would be of no use. Now, if the brain moulds the shape of the bone, it does so at a period when the mind is so utterly undeveloped that no cephalic organs can be the result of its exercise. On the other hand, if the bone moulds the brain, then, as the organs are the development of the exercise of the mind, therefore the mind must be in the bone! and the brain is not the seat, organ, or instrument of the mind—but they started with that statement—"*reductio ad absurdum.*"

VII. ERRORS ARISING FROM PARTIAL INSTEAD OF COMPLETE OBSERVATION.

This is too evident a source of error to render it necessary for us to dwell long upon it. Who that has read the admirable tale of "The Chameleon," can ever forget this cause of fallacy? We have witnessed this error in chemistry, in reference to earth, air, fire, and water. Then again in phrenology—no one who will take the trouble carefully to examine the subject practically, will doubt that not only does not the external plate of the skull correspond with the surface of the brain, but the external plate does not correspond with the internal plate, much less with the brain, between which latter two there is a potential cavity.

If observation be not complete, we almost necessarily conclude erroneously on the subject in hand. It is difficult enough for most people to reason correctly with a perfect knowledge of the facts of the case on which a conclusion is sought; how much more so when part of these facts only is known!

VIII. ERRORS ARISING FROM MISTAKES IN OBSERVATION.

These errors are similar (in effect, though different in the process of evolution) to the last style of error referred to. They may be caused by carelessness, or by what will presently be mentioned, "incapacity for observation."

The mistakes in this respect may be in reference to the facts or foundation of the argument—the motive of experiment—the process of experiment—the appreciation of the experimenter's reasoning—or the real nature of his results. Hence it is that most practical philosophers are not satisfied with one inquiry simply. Hence it is that they vary processes—working upon the same data—in order to see if the results they have obtained are "products" or "educts." Hence it is that they do not "rush to the press" before time and patient study have justified them in publishing to the world not what *may be*, but what *is*.

From this source arise misquotations, and perversions of meanings. An amusing instance of this occurred in a provincial paper which reviewed Dr. Simpson's work *against* homœopathy—as it most decidedly and evidently is—as being an admirable treatise in its favour. To our scientific readers, examples of controversies arising simply from misquotations or incomplete references will at once occur; some mistakes being as ludicrous as the one we have just referred to. How many persons are satisfied with partial quotations, forgetting the grand fact, that to quote —*out* of context—is, in nine cases out of ten, to misquote. These mistakes do not necessarily involve incapacity for this mental exercise, as we have before hinted, but rather impatience of result, carelessness, or prejudice in favour of this or that opinion.

The homœopathists at one time, convinced of the ridiculous figure they made before the thinking part of the community, specially in reference to their decillionths of grains of nothing—as "cogent, potent, and powerful"* doses—seized, by a *mistake* in *observation*, upon a thing one Mr. Rutter invented, called a "magnetoscope." (What's in a name?) This celebrated combination of mahogany, brass, string, and sealing-wax, was equal in effect to the divining-rod of old, or the magic triangles and circles of the Egyptians. Thus writes Dr. King, of Brighton, to Mr. Rutter, the wonderful inventor of this extraordinary instrument.

> "I may be thought too fanciful in the view I take of your beautiful, and, as I think, sublime discovery; but no reflecting mind will deny that we stand in need of some new principle, or truth, to enable us to turn to full account those which we have already received. . . . . When I first saw your machine prove the polarity of a decillionth of a grain of silex, and when I first saw it respond to the billionth of a grain of quinine, I was seized with the same kind of awe as when I first studied the resolution of the nebulæ, and as when I first saw globules of blood and the filaments of the nerves through the microscope."

We do not wonder at this awe-struck "King of thought" being so powerfully affected, for such phenomena are, to the weak, overpowering; while we reflect on the beautiful truth that "milk is for babes," we know that any appreciable amount of milk is stronger than the decillionth of a grain of silex, or the billionth of a grain of quinine! Alas! alas! for Dr. King, the would-be scientific homœopathists have found it essential to give up "magnetoscopic demonstrations," and to leave him in his awe-struck rhapsody, with "silex," "quinine," "male fly's-wings," and his wonderful bump of credulity; and we do not think that any reflecting mind will deny that Dr. King really does stand in need of a great many new truths.

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* This alliteration—so to speak—of ideas seems to be a kind of homœopathic mental convulsion, producing however no accumulative effect, and puts us in mind of the man who, when wishing to be striking in a remark, began, "I suppose, think, and imagine."

IX. ERRORS ARISING FROM INCAPACITY FOR OBSERVATION.

Who told you so?—Who saw it?—Who performed the experiment?—These are most important questions; they strike at the root of belief on evidence. But they should have a reflex influence. Thus, are *you* qualified to observe the point at issue, leaving this for your own decision? We may remark, that we should not consult the premier of England on a case of heart-disease, or a medical man on the suspension of the *ever* "impending crisis" in political affairs.

But besides special adaptations to particular inquiries from pursuits and habits, there are qualities of mind which when possessed enable some to judge of some subjects better than others. Those who possess accuracy, veracity, no love of the marvellous, and whose education enables them to grapple with the subject in hand, are worthy of the highest credence, but these are not the followers of the popular delusions now so rife; and if some men of eminence have graced the heterogeneous ranks of these disciples of the pseudo-sciences, we feel sure that those among our readers who know such persons well, will be able to indicate without difficulty the point where the love of excitement or the thirst for novelty has been able to pervert the judgments of a mind otherwise capable and true.

There are indeed persons so weak and credulous as to be unworthy of all consideration. These are the people who quote as scientific and veritable *facts*, instances of proof from Mrs. Crowe's 'Night Side of Nature.' Most admirable title!—Most unhappy reference! For never, as far as *facts* and *conclusions* are concerned, were either author or readers so much in the dark!

X. ERRORS ARISING FROM INNATE LOVE OF THE MARVELLOUS.

Those who have not been in the habit of exercising their reasoning faculties upon subjects above the ordinary affairs of every-day life—and very few really have—cannot be expected to go through a process of logical induction upon abstruse matters, or matters out of *their* ordinary modes of thought. Numberless things are constantly occurring about them, of which they can give no satisfactory account to others, and indeed upon which they have not satisfied themselves. Cause and effect are to them almost foreign matters; or they are accustomed to resolve all effects into some generally acknowledged and popular cause, which, in nine cases out of ten, is nothing more than a NAME. The more astonishing the name, the more it is apart from their ordinary conception, the more satisfactory the mental process becomes; and they deceive themselves into a belief that they have been *reasoning*.

Instead of arriving, like the real philosopher, at an "ultimate fact," they arrive at an ultimate step only—something beyond which they cannot go, although it is surpassed by hundreds daily. They do not recognise the mental process by which they deceive themselves, and, getting thus confused, shake their heads mysteriously and say with Hamlet, "there are more things in heaven and earth than are dreamt of in your philosophy;" which, if it mean anything, means this: because they are not included in philosophy, they must be believed as facts. This will do for the stage, but not for science. Hence arise lazy belief, the reception of facts, theories, principles, and phenomena on hearsay, and the conviction that any conclusion is better than suspension of judgment. It is one of the highest acquisitions of mind to be able to suspend judgment and acknowledge our doubtful state to others. If more of us did this, there would be much less error in the world. Some people seem to think that there is no position between decision for or against a point—between belief in a principle or statement and disbelief of the same. And others think that if they cannot point out the fallacy in an argument, that argument must necessarily be correct, and its conclusion logical. We would have such people visit some of our courts of law, not to learn the petty quibbling and playing upon words in the *pro* and *con* argument, but to see the state of mind we refer to exemplified—namely, the SUSPENSION OF JUDGMENT until the evidence be perfect. It would at first sight seem that such a decided state of mind upon *all* topics, is a sign of acumen and considerable mental calibre; but this is not really the case. It is the result of absolute laziness, for it is much more easy to decide than to judge or discriminate; the one is merely affirming a conviction, the other is collecting the evidence for that conviction; the one is asserting a principle, the other is tracing its development; the one is talk, the other is work. But men prefer, for the most part, revelation to induction; they are not satisfied, so to speak, with Moses and the prophets, but wish one to rise from the dead. It of course saves a great deal of trouble, and of that style of work to which they are not accustomed.

Now the ready-made sciences—THE PSEUDO-SCIENCES—such as mesmerism, phrenology, homœopathy, electro-biology, and hydropathy, appeal one and all to these qualities of mind. We do not wish to be uncharitable; but look at the followers of these delusions. Some of them, innocent enough, merely, like so many *dilettanti*, amuse themselves most harmlessly; some cure a sore throat in two hours by the decillionth of a grain of nothing; others bless the days they live in when they can hear of the winking virgin of Rimini; others hire their servants by the development of the cranium; others devote themselves to the production of "sweet slumber" by mysterious motions of their arms, hands, fingers, and eyes; while others, most harmless of all, torture themselves with heat and cold, in the vain hope of a coming "crisis." Enjoy your pleasing delusions for a time, illustrious *savans*!

XI. ERRORS ARISING FROM THE SYSTEM OF DISCIPLESHIP.

There are two classes of society—the leaders and the led; they are necessarily disproportioned in numbers, the led proving by far the majority; this, after all, is less from the absence of capacity among the led, than presence of boldness, activity, and industry among the leaders. Many are leaders, not because they possess higher mental qualifications than their fellows, but because the minds in the circles in which they move are sluggish and inactive, and prefer being thought for to thinking, or, rather, *prefer* nothing at all.

There is a moral view of this and the following consideration (XII.) which cannot be too forcibly impressed upon the mind; that is, our accountability for opinions on these subjects, if not to a higher power, which we believe, in common justice to our fellow men. There is a sense in which a man has no right to "enjoy his own opinion"—that is, when he forms it without a proper reason for his conviction, but merely as a whim or fancy, as it must invariably be when he follows a leader simply because he will not seek a path for himself.

After what we have stated, it cannot be supposed that we imagine all men must or may be leaders, or that there is any littleness of mind in being led; we think, rather, that it is a mark of a high and exalted intellect, when, after the application of those tests to this or that argument which are qualified to carry conviction with them, a man boldly affirms his adherence to an opinion, be it moral, religious, political, or scientific. It is not of such we speak; but mark the difference. Is it not notorious that individual opinion becomes merged in sects, schools, and clubs? *Given*, any particular question for a decision; and if you know a man's *clique*, you may generally affirm his decision upon the subject in hand. This is observed less among scientific men than among others, but simply because their questions of investigation are less popular. There are *schools* of science—there are sects among them; there are clubs and coteries—rivalries of the lowest kind. If you will be led by this man or that journal, you may be sure of applause, if not of place. It is of no consequence to them whether or no you think with them, although it may be and is of consequence to science.

As an instance of the errors arising from discipleship, look at the delusion of mesmerism. Who among medical men in this country and in these times would have believed in mesmerism as it is now defined, had they not had a leader, who, at the period of this heresy, was assuredly one of the most popular physicians of the day? It was then, and has been since, on this subject, "follow the leader!" and now mesmeric doctors are as common as if they had any ground for belief in their particular views. Homœopathy and hydropathy have had leaders of another kind—money and interest; while, in most cases, they have had a propulsive ejectment from legitimate medicine, a *vis a tergo* from poverty, and lack both of patience and patients. It matters not what the leader may be, money, fashion, names, men, or systems, let them take the lead, and as long as men do not investigate for themselves with a conscientious regard for truth, and an acknowledgment of the fact that they are responsible for their opinions, the system of discipleship will be a never-failing source of error.

XII. ERRORS ARISING FROM THE SYSTEM OF LEADERSHIP.

A degree of *éclat* attaches to a man who is the originator of this new view, or that particular doctrine. If he have selected his subject well, there is a positive source of profit. There is often a kind of power obtained by this popularity, which in men of little minds and who love to patronize, gratifies even more than the *éclat* of the unthinking audience.

Unfortunately this is so commonly felt and tacitly acknowledged, that men act upon it without questioning its morality, or their own qualifications for the task of leadership. Hence the present fashionable system of taking up *specialities*, particularly in the medical profession; the speciality in nine cases out of ten being taken up, not because of a man's innate qualifications for the thing—not because his pursuits have by force led him to the subject—not because his previous education has been so elaborate and his studies so deep in the numerous, peculiar, and allied sciences of this noble profession—not because his comprehensive genius, grasping the whole range of medicine, sees that here or there he can elaborate a weak point or develop a hidden mystery—but because here or there "there is an opening." Then the journals are *used*—then his conversation swells bigly with laboured thoughts on *his* speciality—then he rushes about among his friends with profound and mysterious hints on the importance of this or that original idea—then he speaks at societies through his peculiar trumpet—then somebody who can't speak either punctuates his large discourse with "notes of admiration," or, as we have noticed more than once, by a "full-stop" in the shape of some quaint remark which is very humiliating; till, not heeding this hint, at length he commits the grand mistake of supposing that he is an authority, because he has obtained the envied goal of his laborious ambition—the dictatorship of a leader!

Nevertheless, specialities are good, but those must be the specialities of special men, not the men who just having finished their studies, or lacking practice, seek notoriety, and look out for "openings" or "riders" into notoriety. To be sure, notoriety they may get, but not fame—a peculiar cab or an extraordinary hammer-cloth will give them notoriety, but it must be a peculiar thought and extraordinary talent that will give them fame. The man who is to be *the* leader, in the sense of an authority, cannot help it—the position is as naturally conceded to him as if he were born to the title; but your *soi-disant* authority, if he have impudence enough, may, meteor-like, flit across the circumscribed heaven of his own little sphere, but as soon disappears from the horizon, and the "place thereof knoweth him no more."

But, also, specialities are bad, in this latter sense, because men have to assume to *be* what they *are not*—they have to put on the *show*, instead of working for the *reality*. And, although they may deceive themselves into the belief that they are "working out one idea," they take with them such *media* of vision in its investigation, that their every thought is tinged by their colouring; so true is it that "the eye sees what it looks for." Hence, not only are they a source of error to the unthinking multitude, but a living deception to themselves.

There may be, and doubtless are, many other specific sources of error *in scientific observation than those now alluded to; but all the general* causes are, we think, here embraced; and if the reader were to carry out their spirit and real application, either in the investigation of original subjects, or in testing any doctrines, systems, or opinions, which may come in his way, there would be very much less error in the world in a few years than at present exists. He would neither use nor admit the usage of words whose meanings were not accurately defined and fixed; he would argue about things, and not names; and would not substitute conventional phrases for absolute realities. He would receive nothing on the testimony of another which he could prove for himself; or in receiving or observing evidence as data of induction, he would reason logically—he would not admit incorrect data, or be satisfied with a logical argument from premises which he had not tested. His observation would be complete and accurate; and thus, in such an exercise, would he gain greater power for further observation. Satisfied with those mental endowments with which his Creator had blessed him, he would seek a high culture and a legitimate use of them, casting away all dealings with the marvellous, and looking suspiciously, rather than otherwise, at anything bordering upon the miraculous. He would not tamely follow a leader because others did, or pin his faith to this or that opinion because of the prestige of names or dates, or the fascination of novelty; nor would he, with a vaulting ambition, seek to be a leader where he had not the power to command respect, and the genius to direct opinion.

Without the bombast and parade of assumed authority, his opinions would be quietly, but nevertheless powerfully, expressed; and, aided by truth, they would work like the "little leaven," influencing society, first in small, and then in large masses, until the sphere of each one's influence would meet, not to clash and rebound, but first to yield, then merge, and then coalesce, in one large sphere, in whose centre would be placed the light of all truth, from which the divergent radii, spreading to the distant circumference, would dispel the darkness of the night of ignorance, and introduce to all mankind the daylight of eternal truth.

Boon Hayes.

REVIEW XI.

Ueber parenchymatöse Entzuendung. By RUD. VIRCHOW. ('Archiv. für patholog. Anatomie und Physiologie u. f. Klin. Medicin.,' vol. iv. pp. 261—321.)

On Parenchymatous Inflammation. By RUDOLPH VIRCHOW.

THOUGH much has been written about inflammation, it must be confessed that the pathology of this form of deviation from health is still far from being sufficiently elucidated. The more we inquire into the various phenomena of inflammation, influenced as they are by the tissue affected, by the constitution, by the exciting cause, &c., the more satisfied are we that it is in many instances impossible to form a line of demarcation between the healthy process of nutrition and that aberration which is called inflammation, or that which leads to the formation of pseudoplasmata, or of hypertrophy, or various other morbid processes.

The author of the article before us appears to be well aware of this *difficulty, and endeavours to study the nature of inflammation by the* phenomena observed in the *elementary components of the various tissues.* In explaining his views, we intend to make use as much as possible of his own words, by which the reader may be best enabled to judge of the value of his researches.

The term *parenchymatous inflammation* is applied by the author to those inflammatory processes in which the characteristic and essential changes are met with in the elementary components of the tissue, without any appreciable exudation taking place, either into the interstices of the tissues, or upon the free surface of membranes. It will be seen, therefore, that Virchow applies the word *parenchymatous* in a different sense to that in which it is generally used. By most pathologists the "parenchymatous inflammation" of an organ is opposed to the inflammation of its lining membranes. Thus, many pathologists speak of a *glossitis mucosa* (inflammation of the lining mucous membrane of the tongue), and *glossitis parenchymatosa* (inflammation of the substance itself); of a *nephritis parenchymatosa*, to distinguish it from the inflammation of the mucous membrane, (nephritis mucosa, generally called *pyelitis*); and from inflammation of the external serous membrane (nephritis serosa, or *perinephritis*). According to Virchow the inflammation of the lining membranes of organs may be as well parenchymatous as that of the substance of the organs (generally called the parenchyma) itself; to be parenchymatous inflammation it requires only that the principal changes shall take place *within* the elements themselves, without exudation *into* the interstices, or *upon* the free surface. We shall return afterwards to the consideration of the propriety of this expression of parenchymatous inflammation. At first we must briefly state his views on the intimate nature of inflammation. We cannot fulfil this better than by quoting from an earlier article "On the Dilatation of the Small Vessels."

> "Some have endeavoured," he says, "to attribute the origin of inflammation to the vessels, others to the nerves, others to the tissue, while disputing about the outset of the phenomena, they have neglected the question about the intimate nature of the process. Doubtless all the factors on which the nutrition of a part depends, must have their share in its inflammation, the blood as well as the nerve; the nerve as well as the membrane of the vessels, as the tissue. As soon as real inflammation is established, all elements must participate. This diseased state of all the constituents of a part may be originated by any one of the single factors of nutrition; the blood as well as the nerve, as the vessel, as the tissue, may form the prime mover of the inflammatory disorder, which later influences the relation between all of them, and which is not to be considered as inflammatory unless *all* the factors are participating, as without this there would exist merely *hyperæmia, neuralgia*, &c., but not real *inflammation*" (Arch. f. path. Anat. and Physiol. iii., p. 459).
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> "If, therefore," continues the author in the present essay, "inflammation is to be considered as a diseased state of all the constituents of a part, it can, of course, not be permitted that affections of merely a single constituent, as hyperæmia, neuralgia, exudation, metamorphosis of the tissue, shall be considered as inflammatory." (vol. iv. p. 279.)

We must abstain here from entering fully into this question; it appears to us, however, difficult to prove the view of the author, that in the above processes considered by him as elementary (hyperæmia, &c.) only one element or constituent is affected, and that metamorphosis of the tissue can take place without a simultaneous or previous affection of the vascular constituents and the nerves of the same part, or without an altered relation between them.

We need only shortly mention that Virchow does not consider the inflammation of a part as a perfectly new or specific process, but, like every other local pathological process, only as an aberration of the normal nutrition. In this respect he is, therefore, in accordance with our older pathologists. He sees in the various phenomena of inflammation "the excess of all or of certain single processes of nutrition." (p. 275.) The healthy state of nutrition is understood to be conserved by the equilibrium between two currents of fluid, the one going from the capillaries to the tissue, the other from the tissue to the capillaries; or, to use with him a short though not quite accurate term, in a certain state of diffusion between blood and tissue. The acts of absorption and of exudation, which represent these two currents, are considered as being under the constant influence of the nervous system.

> "If we suppose this," he says, "we find in analyzing the phenomena of inflammation—1. That the equilibrium in the exchange between blood and tissue is disordered in such a manner as to produce either increased absorption, or increased exudation, or both together; 2. That the nerves of the diseased part are in the state of excitation or irritation. If the absorption is increased, it must lead to inflammatory atrophy; if the exudation is excessive, inflammatory tumefaction must be the consequence; if, at the same time, exudation and absorption are in excess, we find the elements of the tissue to become atrophied, while in their place exuded matter is deposited, which, at first amorphous, undergoes afterwards some one or another metamorphosis."—(pp. 275, 276).

The matter exuded during inflammation may be deposited either into the interstices between the elements of the tissue, or on the surface of the tissue, or into the elements themselves. According to this threefold possibility, Virchow makes the subdivision into inflammations with an interstitial, with a free, and with a parenchymatous exudation. He does not, however, deny the possibility of the co-existence of two of the forms, or even of all three, in the same morbid process. We are inclined to think that rarely one of the forms does exist alone. We can scarcely imagine an inflammatory effusion to take place upon the free surface without any such participation into the elements of the tissue, or into the interstices, nor does it appear to us possible that an anomalous fluid is effused into the elementary constituents without a simultaneous or rather preceding effusion into some kind of interstitial space between the blood-vessel and the elements through which the normal as well as the abnormal nutrition takes place.

As one of the best examples for the explanation of the phenomena of the parenchymatous inflammation, Virchow describes in detail the *inflammation of the muscles*, repeating the words he made use of in 1847, in a lecture before the University of Berlin.

> "The inflammation of the muscles," he says, "is very similar to that of the kidney commonly called *Morbus Brightii;* the only difference between them consists in the circumstance of the muscles being provided with a greater quantity of interstitial tissue. In the inflammation of the muscles we find, therefore, the exudation either only in the interstices between the primitive fibrillæ, or simultaneously in the interstices and in the fibrillæ, or only in the fibrillæ. To com-

mence with the last-named form, we see, if it is of acute source, at first *a change of colour and cohesion* of the flesh of the muscle,—phenomena which both must make us think of a change in the molecular composition of the tissue. The flesh assumes in some cases a violet-blue colour, in some a greyish-red and brown, in others it approaches a pale white, yellow, or green; it is brittle, and easily torn into pieces. The microscope shows that the ultimate fasciculi of the fibrillæ take at first a more homogeneous appearance, their transverse striæ become indistinct, they easily break into irregular fragments, and at their ends these fragments are frequently seen to divide into longitudinal fibrillæ. Later the contents of the ultimate fasciculi become still more dingy, lose the yellowish colour, and turn still more grey; within their sheaths gradually a molecular substance is observed of great density, which, by acetic acid, becomes clear, and exhibits all the characteristic reactions of protein; here and there, also, a fat-globule may be observed in it. In the most acute course, the connexion between the primitive fasciculi is soon destroyed, the sheath is ruptured or dissolved, the molecular contents flow together into one cavity; the muscle is said to be in a state of *inflammatory softening.* If the case is less acute, we see within the molecular substance at first, some globules of fat, the number of which is gradually increasing, until the whole fasciculus appears filled with fine granules of fat, connected by a scanty azotised substance. Not rarely the whole series of these changes may be observed in one single fasciculus. At last we meet with cases, and these too may yet be of a comparatively short course, in which the fatty metamorphosis takes place without a previous decay of the primitive fasciculus into molecular matter. In these cases we observe the fat-globules arranged in neat and pretty rows, one placed behind the other like a string of pearls, in the longitudinal axis of the fasciculus, exactly corresponding to the longitudinal fibrillæ of the muscle,—a fact which affords a new proof for the universality of the law of the fatty metamorphosis in the azotised components of the body.

"Besides these there are other forms of a slower course, and with a different termination. Some of these cases exhibit the fatty metamorphosis of the fibrillæ; in others, however, the visible phenomena are not striking. To the naked eye the colour of the muscle appears changed, somewhat of a greenish yellow or whitish yellow; under the microscope the ultimate fasciculi are not any longer yellowish, but perfectly (?) colourless, while within them small groups of glittering molecules of a yellowish colour are seen; the whole has the appearance as if the colouring matter of the muscle, which formerly had been equally diffused over the entire substance, were collected into granules deposited only on some spots. The ultimate fasciculi, at the same time, more easily break into pieces than in their healthy state, their transverse striæ have become indistinct, the longitudinal fibrillæ make their appearance without previous preparation. But in whatever manner the metamorphosis of the ultimate fasciculi may vary in the chronic course of the local process, after some time increased absorption always takes place, the changed contents of the fasciculi are re-absorbed into the circulation, and then sometimes a loose tissue of a rather fibrous nature is observed, with a great number of longish and oval nuclei. Later the spot formerly affected appears shrunk below the surface of the surrounding part; it is occupied by a more or less dense reddish or white areolar tissue (*Bindegewebe*—connecting tissue), which exhibits at the termination of the process a splendid, tendon-like appearance—*the tendinous macula* (*Sehnenfleck*) *of the muscle.*

"As we have stated already, these may be the only changes, or they may be connected with an exudative process into the interstitial cellular tissue of the muscles. These combined forms are well described by *Gendrin.** We restrict ourselves to mentioning here, that the exuded substances may consist in some cases of an albuminous, in others of a fibrinous, in others of an hæmorrhagic fluid. These exudations may proceed in their metamorphosis, and it is principally putrefaction (*Verwesung*) or *abscess of the muscle* they generally lead to." (pp. 265—268).

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* **Histoire des Inflammations. 1826.**

As to the circumstances and clinical symptoms under which this morbid process takes place, Virchow mentions that the cause generally known is of a traumatic nature, but that very often also the so-called *rheumatic pains* have their origin in an inflammatory state of the muscles, and that most diseases accompanied by rheumatic symptoms lead to changes in the muscular apparatus.

The best specimens of rheumatic inflammation of the muscles the author met with in the muscle of the heart.

> "You observe," he says, "the flesh itself grow pale, while there exists at the same time a moderate hyperæmia of the interstitial vessels; the red colour of the flesh changes, by degrees, into a greyish red and yellowish red, lastly into a dirty yellowish white or greenish white, while you see under the microscope, within the ultimate fasciculi, the alteration just described. If the process proceeds further, retaining its acute decourse, the change of colour advances more and more, the tissue becomes so brittle that it may lead, as I have witnessed in one instance, to rupture of the heart. In general, however, the process assumes the chronic form, you see the ultimate fasciculi in some places entirely disappear, the spot becomes sunk in or contracted by a callous or tendinous cicatrix, a change which already *Morgagni* had described as *ritium carnis cordis in tendineam naturam degenerantis.* (Ep. xlv. Art. 23)." (p. 270).

As another morbid condition of the system with which muscular inflammation leading to tendinous cicatrices (*schwielige Muskelentzuendung*) is frequently connected, Virchow mentions secondary *syphilis*, which differs from the rheumatic affections, principally by the constancy with which it persists in one single or only a few muscles.

Another instance by which Virchow's meaning of "parenchymatous inflammation" may be explained, is given by his view of the intimate nature of Bright's disease, as being a parenchymatous inflammation of the kidney.

> "In another place," he says, "I have already demonstrated that, while in the canaliculi recti and a part of the canaliculi contorti, fibrinous cylinders (i. e., free inflammatory exudation) are found, those changes which give rise to the characteristic anatomical condition of the kidney, must be looked for only in the epithelial cells of the canaliculi contorti. In the first stage these cells enlarge, and their molecular contents increase. In the second stage this increase may reach such a degree that it leads to the complete breaking down of the cells, in consequence of which the canaliculi appear filled with a molecular, albuminaceous substance, a state at Vienna detached from the *Morbus Brightii*, under the name of *lardaceous infiltration.* The cells may also undergo the fatty metamorphosis, they may become filled with minute granules, forming thus the *stearosis*, while the aggregated fat-globules (*Fettaggregatkugeln*) represent the long-known exudation corpuscles (*Entzuendungskugeln*). In the third stage all of these aggregations of oil granules break down, an emulsive fluid is formed, which, during the development of the fourth stage, is either absorbed or excreted with the urine. In some cases the whole process appears entirely confined to these changes in the epithelial cells, no free fibrinous exudation being poured out into the cavity of the canaliculi. Then all the exudation takes place into the elements of the tissue, except the albumen which is carried off with the urine." (p. 265).

We cannot enter in this place into the pathology of Bright's disease, but concerning our present subject, Virchow's own words appear to us sufficiently to show, that the expression of "parenchymatous inflammation," in the author's meaning, is not well chosen for the disease in question. The circumstance that no free fibrinous exudation is poured out into the canaliculi, does not entitle us to conclude that the whole morbid process is confined to the epithelial cells. But the fact of albumen being poured into the canaliculi, and passing off with the urine, proves that *not* "all the exudation takes place into the *elements* of the tissue," but at least also on the free surface, if not, at the same time, into the interstices too.

There are, however, organs in which we can detect scarcely any palpable interstices; in these, therefore, the inflammatory changes can be observed almost only within the elements of the organs. Such is the case with the cornea, the cartilage, the bones, &c. In the *cornea* Virchow created inflammation by the application of intense caustics and various stimulating agencies. The changes manifested themselves at first in the cells of the cornea, which became larger, exhibited some fat-globules, and an increase of their nuclei in size and number, as described already in Bowman's Lectures. At a later period the intercellular substance became dim, opaque, more dense, easily dividing into single fibrillæ, approaching, in its appearance, that of the sclerotica, sometimes exhibiting a granular aspect as if covered with dust, and likewise displaying oil-globules under the microscope. The process may be arrested here, and give rise to the various degrees of opacity (Leucoma &c.), or it may lead to softening of the substance of the cornea (Keratomalacia) and superficial ulceration. But in no case did Virchow find any free exudation between the lamellæ of the cornea, or between the single fibres. The inflammatory process in the true cartilage is described as very similar to that in the cornea. The cells become larger, the number of the nuclei increases, some or all of them may undergo fatty metamorphosis, while the intercellular hyaline substance becomes fibrous, divides into filaments, and assumes the appearance of a more or less soft, gelatinous, cellular tissue. Virchow's observations on this subject are, therefore, almost entirely in accordance with those of Ecker,[*] Goodsir,[†] and Redfern.[‡]

Some, perhaps, maintaining the view that inflammation cannot take place in tissues which possess no bloodvessels within themselves, but only in their circumference, will object that the above-described changes in the cornea and cartilage ought not to be adduced as specimens of inflammatory alterations. But the progress we have made in understanding the process of healthy and unhealthy nutrition, takes away the force of such an objection. The circumstance that no bloodvessels are distributed within these tissues, does not give rise to a real difference in the process of their nutrition. There are no tissues the elementary components of which are in a perfectly *immediate* connexion with the blood; the distance between both is greater in some, less in other tissues. Organs in which we find the former arrangement (i.e. those with few bloodvessels, or none, within their substance) possess a less rapid change of matter and a greater independence of constitution, than those which in all their parts are richly provided with capillaries; but physiological experiments as well as pathological facts prove that the former are likewise in a constant change; we see them partake in the morbid processes of other organs; we see their nutrition deviating from the standard in various modes—why then should that aberration be excluded which we call inflammation? And if we observe that in these bloodless tissues certain changes are produced by the application of those stimuli which are known to excite inflammation in the vascular tissues, changes which are, *cæteris paribus*, identical in both tissues, why should we not use the same term for both processes? It is true that changes in the cornea and cartilage, similar to those which we call in some instances inflammatory (if produced by stimuli usually exciting inflammation, &c.) are met with in the so-called alterations of old age. But this is no proof against the inflammatory nature of the former; these alterations of old age are found also in tissues provided with capillaries, and also in these they are like the changes produced by inflammation. We know that the same change may be effected by various means, that it is not the result by itself from which we can always conclude on the nature of the process.

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[*] Archiv f. physiol. Heilkunde. 1848, Band ii. p. 235 ss.
[†] Anatomical and Pathological Researches. Edin. 1845.
[‡] Monthly Journal of Medical Science, 1849, 50.

In treating of the inflammatory affections of the bones, Virchow distinguishes from the true *ostitis* (the parenchymatous inflammation) those conditions which are connected with exudation on the free surface of the bone, or into the interstices between the elements of the tissue (*periostitis* and *endostitis*). In the inflammation of the *substantia ossis propria* the first change observed by him frequently consisted likewise in the fatty metamorphosis of the cells of bone. Groups of small oil globules made their appearance in the cavity of the cells, and single ones sometimes also in their tubuli. At the same time, often also without this change, some larger cells were seen, and in rare instances these had a double nucleus.

> "On inflamed ribs," he says, "but principally on the lower articular surface of a carious tibia, I saw at some distance from the exterior surface of the cells the commencement of a disjunction of substance. Some almost completely round, only slightly oval, bodies become disconnected from the intercellular tissue, exhibiting the appearance of enlarged cells of cartilage provided with tubules, being separated from the surrounding still homogeneous substance by only a very thin fissure. At the same time other formations were seen, already more disunited, in which also the shape of the cell was much changed; nothing but rather soft, mouldy, granular aggregations could be detected, which exhibited indistinctly the tubules here and there filled with the most minute oil globules—rarely the osseous corpuscle itself was seen. By washing these, pulpy masses could be removed, after which process the superficies of single pieces of bone offered the appearance so well described by Howship, i.e., roundish cavities, on the one side more or less open, on the other more or less provided with a wall of not yet softened osseous tissue, 'as if they were chiselled out by a half-round chisel.' Amongst the more generally known microscopic objects, the margin of a cut through the pulmonary vesicles would give the best idea, though in size rather too large." (p. 303.)

The most interesting phenomenon in this process is, that the osseous tissue does not undergo the alteration in an uniform manner, but in single partitions, each of which represents the province of a single corpuscle of bone, a circumstance highly illustrative of the function which these elements perform in the nutrition of the bones. The principal product of this inflammatory process is softening, and later, rarefaction of the osseous tissue; we observe in this respect much resemblance with those morbid conditions which we in general comprise under the head of *osteomalacia*. While C. Schmidt and C. O. Weber consider the rarefaction of bone in osteomalacia to be the effect of a chemical solution of the calcareous matter by means of a free acid, Virchow is of opinion that parenchymatous inflammation of the bone is the intimate nature of the disease.

Very similar to the alterations of the *cornea* and *cartilage* are those of Virchow's "parenchymatous inflammation of the *areolar tissue.*" They may be seen in the circumference of inflamed tissues, for instance the skin, where both phenomena are displayed, the fatty metamorphosis of the fibres and the multiplication of their nuclei.

In those tissues which are principally composed of cells, the inflammation of the parenchyma, in Virchow's sense of the expression, might be almost called an inflammation of the cells. As an instance of this we have mentioned already Virchow's parenchymatous inflammation of the kidney. Another is given by the author in the lobular inflammation of the liver. He is inclined to think that, on account of the change of colour and the whole physical appearance (to the nude eye) of the parts affected, they are not rarely considered as lobular abscesses, while they contain no pus, but merely the changed elements of the tissue (principally the glandular cells).

After having briefly considered the morbid conditions which Virchow describes in illustration of the *parenchymatous inflammation,* we may judge of the propriety of this expression. We must conclude that our author understands by the word *parenchyma* the constituent elements of a part in contact with the interstices between the elements, and with the free surface beyond the lining membrane. Parenchymatous inflammation would therefore be opposed to interstitial inflammation and inflammation on free surfaces. But he certainly would not admit the expression "inflammation on the free surface." It is evident that he uses his term only in consideration of the exudation which takes place during the process of inflammation, into the elements themselves, and into the interstices between them; or, we would rather say, in consideration of the inflammatory changes in the elements, in the interstices, and on the free surfaces. But we cannot think it correct to call the inflammatory process with alteration of the elements themselves, parenchymatous inflammation. Even if we were to adopt, with the author, the word *parenchyma* as signifying the elementary constituents in contact with the interstices between them, yet we could speak only of "inflammations with parenchymatous alterations," not of "parenchymatous inflammations." Virchow's expression might lead also to the erroneous idea, that the morbid process was going on merely within the principal elements (muscular fibrillæ, cells of bone, &c), without affection of all the constituents of a part, though our author himself says, in several places, that inflammation cannot exist without participation of as well the blood as the membrane of the vessels, as the nerve, as the tissue.

We are inclined, therefore, to retain the expression "*parenchymatous inflammation*" in the old sense, i.e., signifying the inflammation of the parenchyma of an organ, in opposition to that of its lining membrane, and to consider Virchow's "parenchymatous inflammations" as inflammations with principal affection of the constituent of the tissues. But though we do not adopt the expression chosen by Virchow, yet we cannot but observe, that every one may gain much information by carefully perusing the author's essay on parenchymatous inflammation. Some will blame him, perhaps, for attributing too much importance to the anatomical, and too little to the clinical facts. While the anatomist pays his principal regard to the changes produced by inflammation, the physiological pathologist inquires after the intimate nature of the process which gives origin to these changes. Though it must be confessed, that in the present essay Virchow has entered but little into the question *how* the changes take place, yet this does not prove that he neglects the study of the nature of the process. On the contrary, the only purpose of the whole of his treatise is to throw light on this subject; the way only which he has chosen differs from that generally adopted; instead of entering into *theories* about the influence exercised by the blood on the vessel, or the nerve, or the tissue, he examines what he can *see*, the change effected *by* and coincident *with* the morbid process. Some, perhaps, will say, what Virchow describes is not inflammation itself, but only one of its events—*exudation*. But can we any longer maintain the view that exudation is not an intrinsic part of the process of inflammation, that it is a mere consequence? If we ask how does the normal nutrition take place? we can but answer—by means of constant exosmosis and endosmosis, of constant exudation and absorption. During inflammation the nature of these processes remains the same, only the factors (blood, vessel, tissue, &c.) are altered. Therefore, as exudation forms an inseparable part of normal nutrition, as well as of inflammation and other varieties of abnormal nutrition, the alterations which Virchow describes in the elements of the tissue have been overlooked, or considered as secondary, by the majority of observers, and yet they are of greater importance for the understanding of the intimate nature of the process than the more palpable phenomena (the abundant exudation into the interstices between the elements and into the cavities); indeed, the latter are apt to make us forget the important fact, *that inflammation is only one of the various shades of deviation from the normal process of nutrition.*

In the present, as in any other of Virchow's essays, the reader will meet with an abundance of new ideas, worthy of the most careful consideration; he will find in every page sufficient proof of the author's energetic desire to eradicate all views and theories which are not borne out by indisputable facts; everywhere is seen the ardent contest against what he considers to be the general character of medical writings—a mixture of arbitrary rationalism with the most rude empiricism (*Gemisch von willkürlichem Rationalismus und crassem Empirismus*). The only way approved of by Virchow is that pursued in physics and other branches of natural science, which in medical science is that of unprejudiced observation of the healthy and morbid processes under various influences, and in their various stages and modifications. That he has met with full approbation cannot remain doubtful, if we consider that his authority is generally acknowledged to be of the highest order in most of the branches of scientific pathology, though scarcely ten years have elapsed since he finished his university studies. Still young in years, he is a veteran in observation, and few men have been equally fortunate in discovering new realms for science, and in avoiding the quicksands of imperfect observation.

REVIEW XII.

Elements of Psychological Medicine; an Introduction to the Practical Study of Insanity, adapted for Students and Junior Practitioners. By DANIEL NOBLE, M.D., F.R.C.S., Medical Officer to the Clifton Hall Retreat, &c.—*London*, 1853. Post 8vo, pp. 340.

THIS little volume contains Eight Lectures on Insanity, which were delivered by Dr. Noble during last session, at the Chatham-street School of Medicine, in Manchester, and which he has published at the request of his medical brethren, who constituted a large proportion of his auditory. We happen to know that these lectures were listened to with great interest by several of the most distinguished members of the profession in that city; and that the request for their publication was not an idle compliment, but a genuine expression of their sense of the value of what they had heard, and of their desire that others should benefit by it. To the valuable recommendation of the book which this fact conveys, we are glad to find ourselves able, after a careful examination of its pages, to add our own very favourable testimony. Dr. Noble does not put it forth as a systematic treatise on the subject, but designs it merely as an introduction, arranged upon a plan that shall take the student up, as it were, at the point where the completion of his lectures and hospital attendance leaves him. He has connected the pathology of mental maladies, as much as possible, with the present state of our knowledge of cerebral physiology; adopting in nearly every particular, the views enunciated by Dr. Carpenter, in his reviews of "Noble on the Brain," in our twenty-second volume; and abandoning, with a candour most creditable to him, the greater part of the phrenological doctrines which he formerly upheld, but which his more matured judgment and larger experience (especially in regard to insanity), make him regard as no longer tenable. And he has explained the principles of treatment by constant reference to those more general ones, which are applicable in the ordinary practice of medicine, and with which, therefore, the advanced student, as well as the practitioner, ought to be acquainted.

In his preface Dr. Noble makes some very pertinent remarks regarding the ignorance which prevails through a large part of the profession, in regard to the diagnosis and treatment of insanity. "Well educated persons out of the profession," he truly remarks, "will frequently judge as soundly concerning the condition of a patient, as many who are within it." Our hospitals, colleges, and universities make no provision for training the future physician or surgeon in a practical knowledge of insanity, so that men who possess adequate information upon this branch of their duties, are necessarily self-educated to a great extent. And yet any two practitioners, having no special knowledge of the disorder, are empowered by law to take away the personal freedom of any individual, by certifying to the fact of his insanity; a fact about which those who have no familiarity with the disease are peculiarly liable to be deceived, the most accurate diagnostic powers being often required even by the most experienced.

The first lecture contains a general *introduction* to the study of insanity, and embodies Dr. Noble's view of its essential nature. He defines it as consisting in "chronic disorder of the brain, inducing perversion of ideas prejudicial to, or destructive of, the freedom of the will;" each point of this definition being more fully discussed in subsequent lectures. Thus, after giving in the second lecture such a sketch of the present state of our knowledge of the functions of the brain and nervous system, as may serve to make his readers comprehend their probable connexion with the healthful activity of the mind, he proceeds in the third lecture to discuss the *General Pathology of Insanity;* and under this head he makes good his position as to the essential connexion between this disease and cerebral disorders. His arguments on this point are extremely acute; and we can only regret that he did not fortify his position a little more, by showing how cerebral disorders, that may leave nothing for the morbid anatomist to discover, may be excited either by perverted blood, or by morbid sympathies with other parts of the system. These points, it is true, are not altogether passed over; but they scarcely receive the prominence to which they are entitled. The fourth and fifth lectures embrace an account of the *Varieties and Particular Characteristics of Insanity:* the primary forms of the disease recognised by Dr. Noble being the *notional,* the *intelligential,* and the *emotional.* In the first there is a primary perversion of ideas, giving rise to settled illusions, of which the mind cannot get rid by any effort of its own. In the second there is a primary perversion of the reasoning faculty, quite irrespective of any fixed or characteristic illusion. The term *emotional* insanity is not used by Dr. Noble in precisely the sense in which it is employed by many recent writers; since he refuses to admit into the category of insanity those forms of mental disorder which do not involve some amount of perversion of ideas,—a limitation in which we cannot accord with him,—and applies the term emotional insanity to those cases in which, while the prominent derangement obviously consists in disorder of the emotions, there is at the same time some perversion of ideas.

The sixth lecture treats of the *Diagnosis, Prognosis,* and *Etiology;* the seventh lecture gives a general view of the *Exciting Causes and Physical Treatment;* and in the eighth lecture the principles of the *Moral Management* of the insane are discussed,—the whole being summed up in the following admirable formula:

> "Deal with the physical characteristics which may accompany insanity, as you would deal with them under other circumstances, and act in correspondence with sound principles of medical practice, always remembering that, with high nervous susceptibility, depletion will be but indifferently tolerated, more especially when the ailment refers itself to causes exclusively psychical. For the relief of insanity itself, properly considered, trust almost entirely to hygienic and moral treatment; withdrawing circumstances likely to aggravate the special features of individual cases, and supplying to the mind such objects of attention and excitants to activity [and, we should add, such motives to self-control], as may be best calculated to arouse and sustain a new and more healthful mode of operation."

Holding it, as we do, to be the imperative duty of every practitioner to qualify himself to give a sound diagnosis, and to direct the treatment in any case which may present itself to him, and feeling sure that without some special guidance even those most generally well informed may be led into grievous error, we have great satisfaction in being able warmly to recommend Dr. Noble's little treatise, as extremely well adapted to supply the desideratum which we have long felt and lamented.

PART SECOND.

Bibliographical Record.

ART. I.—*Memoirs of John Abernethy, with a view of his Lectures, Writings, and Character.* By GEORGE MACILWAIN, F.R.C.S. Vols. I. & II.—*London*, 1853.

THESE volumes would give us an opportunity of discussing two interesting points—viz., the character of a great man, and the portrait drawn of him by his biographer—did not the mass of books we have to review warn us that a lengthy dissertation will not be in our power for some time to come. But the volumes themselves will, we hope, be perused by all our readers. They are extremely interesting, and not only give an account of Abernethy, which cannot fail to be read with benefit, but they discuss incidentally many questions of medicine and of medical polity. It may be said, indeed, that sometimes Mr. Macilwain is less biographer than essayist; but this is a fault we can easily pardon. Abernethy's life and character have given him a theme, and in illustrating it he has thrown in something of his own. Indeed, it was difficult to avoid this. In connexion with Abernethy and St. Bartholomew's, the tempting subject of the hospital system could not be passed over. The obligation of our science to Abernethy, naturally led to a consideration of the present position and prospects of the medical art and profession. On both these topics, Mr. Macilwain has a good deal to say, and he has said it well.

The moral character of Abernethy, as depicted in these volumes, strikes us as a just one, as far as we may be permitted to judge without personal knowledge of that remarkable man. Too much is made, we think, of the eccentricity and roughness which are (and, no doubt, in part truly) attributed to him; but the true benevolence and worth of that sterling character are drawn with a faithful pen. Nature had gifted the heart of Abernethy with a rare tenderness and generosity; she had placed on the surface only, the ruggedness which springs from mental disquietude, and from failing health.

We are less satisfied with the intellectual portraiture; and here, we think, in making his work a popular one, Mr. Macilwain has not done justice to his own powers of analysis. Abernethy was no Hunter, and he was no Lawrence; his character, perhaps, was in some respects both inferior and superior to his teacher and his successor. We cannot venture to do so, but Mr. Macilwain might have advantageously contrasted him with these two great men; the study of the various proportions of depth, subtility, and clearness of mental vision in the characters of each, would be very interesting.

Mr. Macilwain is fond of anecdotes, and has inserted a great number; this does not render his work less pleasant reading. We recommend it most strongly, as an interesting, and, at the same time, instructive treatise.

ART. II.—*A Text-book of Physiology.* By VALENTIN. Translated and Edited by WILLIAM BRINTON, M.D. Part II.

THIS part concludes Professor Valentin's great work, and finishes Dr. Brinton's labours. The book will now take its rank in this country as a standard authority on physiology. Its chief characteristics are its abundance of facts, and its conciseness of expression. Scarcely ever is there a word too much, and in this lies its merit. The work consists of 2291 paragraphs, which occupy 669 closely-printed pages. It is illustrated by numerous diagrams and illustrations, which have been very carefully done, and a copious index makes reference easy. In its *getting-up*, therefore, as well as in its proper essence, so to speak, the work is equally commendable.

At the same time, this treatise is not without some drawbacks. The facts are communicated with great clearness, it is true, but with equal dryness. In his power of interesting the reader, as well as in elucidating an obscure subject by detail and illustration, Professor Valentin is by no means equal to Dr. Carpenter; nor, we believe, will his work ever be so generally liked as this latter writer's late masterly treatise. To teachers, and to the higher class of students in physiology, Valentin's work will certainly be very useful, but it is not the kind of book which the commencing student can properly handle.

Dr. Brinton appears to have performed his laborious task with great care and judgment. The notes he has occasionally appended show his own knowledge of the point in question to be great, and that he has thought profoundly on his chosen subject.

ART. III.—*On the Advantages of the Starched Apparatus in the Treatment of Fractures and Diseases of Joints; being the First Part of an Essay to which the Council of University College have awarded the Liston Clinical Medal.* By JOSEPH SAMPSON GAMJEE.—*London,* 1853. pp. 89.

THIS is a practical work, intended to show the beneficial effects of the treatment of fractures by the starched apparatus, as employed of late years by M. Seutin. This apparatus is very simple: splints are made from pasteboard soaked in water, and are then covered both inside and out with a thick coating of starch; the limb is bandaged with a roller covered with wet starch, and the splints are then moulded on the limb, all depressions and tuberosities in which are filled or protected with cotton-wool. An outer bandage, also covered with starch, is then applied, and the limb is kept quiet until the whole is dry, which occurs in about 36 hours. When dry, the bandages are slit up in order to see that the application of the splints has been properly performed, and if any swelling of the limb requires a little loosening of the bandage, or if any shrinking of the bandage requires a little tightening. This being done, a bandage starched on the outside is reapplied, and after this has dried, the patient may leave his bed. The great advantages of this plan are, that uniform pressure is applied, reduction is maintained, and confinement to bed for a long period (as common in fractures of the leg, thigh, and femoral neck, treated on other plans) becomes unnecessary. Mr. Gamjee also states that swelling from extravasation of blood, or from inflammation, need not prevent the application of the apparatus if it be judiciously used; and that if the fracture is a compound one, the only difference of treatment is that the wound may be left uncovered by cutting a piece out of the splint.

In support of these assertions, 17 cases treated in University College Hospital are related, and some judicious remarks are attached to each. Without going into their analysis, or into the details of the manipulation required for each particular fracture, we may observe that the evidence, as far as it goes, is satisfactory.

In succeeding chapters Mr. Gamjee recapitulates the advantages derived from the use of the starched bandage, shows its applicability to some cases of diseased joints, and finally gives a short retrospect of the history of the practice.

The work is well written, and will prove useful, but we must remark that Mr. Gamjee appears to be under a misapprehension when he supposes that the merits and demerits of the starch apparatus (such as they are) are not well known to English surgeons, and have not been commented upon by English writers.

Art. IV.—*Sketch of the Operation and of some of the most striking Results of Quarantine in British Ports since the Beginning of the Present Century.* By Gavin Milroy, M.D.—*London*, 1853. Small 8vo, pp. 38.

This is the re-issue of a paper published in the Association Journal. Like all Dr. Milroy's writings, it is an able paper, but to our minds somewhat fragmentary and inconclusive. The authorities are not quoted with sufficient precision, and the account of the epidemics of Yellow Fever at Gibraltar, &c., is very meagre and, we must say, unsatisfactory. We attach little importance to the voluminous controversy which has taken place on those outbreaks, but if the subject be touched at all, it should be discussed fully. The subject of the *Eclair* fever is only alluded to, and the important question of importation into Boà Vista is passed by. Dr. Milroy may allege that it was not his purpose to do more than consider the subject as it affects British ports, but this limitation of the subject is much to be deprecated. In this way the most cogent example of importation is omitted, and we are called upon to draw a conclusion from the limited experience of a single country.

While we thus express our dissent from the mode in which Dr. Milroy has treated his subject, we fully agree with him that the quarantine system is full of inconsistencies, and requires remodelling. But remodelling is not sweeping away, and quarantine, based on what appears to us an indisputable truth, may be faulty in administration, but must be right in principle. We have always deeply regretted that the Sanitary Reformers, who have really done so much to enlighten the public, and to pave the way for great improvements, should have embarked in a crusade, a hopeless crusade too, against quarantines. Some evil spirit has prompted the word "abolition" instead of "reform," and has, by the unhappy suggestion, cut professional support from under their feet, and will eventually shake the popular faith in their principles and their doctrines. Attempts may be made in periodicals of general literature to influence the public mind, but in the long run the medical profession must decide on a medical question. We are happy to see that Dr. Milroy does not entertain the extreme views adopted by some of his colleagues, and we trust that his knowledge and good sense may eventually lead to a modification of the position assumed on the subject of quarantine by the sanitary authorities of this country.

ART. V.—*Sandgate as a Residence for Invalids.* By GEORGE MOSELEY, M.R.C.S. Eng., &c.—*London and Sandgate,* 1853. 8vo, pp. 136.

IN spite of the comparative ease with which individuals requiring a mild climate may be sent to the south of France, Italy, Spain, or Madeira, every practitioner knows that, in many cases, there are reasons connected with the period of the disease or with the circumstances of the patient, which may induce him to select, if possible, some favoured locality on our own coasts. Hastings, Torquay, the Isle of Wight, &c., are, in many respects, better fitted for invalids than France or Italy. English comforts can be procured, and good English medical attendance is at hand. The vicissitudes of the climate, too, such as they are, are known, and the invalid is never astonished, as at Nice or Montpellier, by the bitter blast of some ice-laden wind, colder far than any he has ever shrunk from in his northern home. To the popular places of resort on the south coast, Mr. Moseley desires to add the name of Sandgate, in Kent. He has written a little book, giving first an account of the town of Sandgate, and of its climate, and then branching off to a general essay on consumption and hygienics. This latter part is evidently intended for the *plebs,* and although true enough as far as it goes, and sensibly written, need not detain us. We turn with much more interest to what Dr. Moseley has to tell us on the climate of Sandgate. He informs us, in the first place, that "sea fog" is comparatively infrequent—no trifling advantage, as those who know Hastings or the Undercliff will acknowledge; then he asserts that, compared with the Undercliff and Hastings, Sandgate has an advantage in the prevailing winds.

"On an average of years," he writes, "there are 24·12 days of north wind in Sandgate, 24·46 in the Undercliff, 62 50 in Hastings; of north-east, 45·80 in Sandgate, 54·61 in Undercliff, 43·50 in Hastings; of east, 37·69 in Sandgate, 60·34 in the Undercliff, 34·50 in Hastings; of south-east, 33 24 in Sandgate, 18·85 in the Undercliff, 15·50 in Hastings; of south, 27·53 in Sandgate, 26 72 in the Undercliff, 17 in Hastings; of south-west, 75·31 in Sandgate, 96·97 in the Undercliff, 137·50 in Hastings; of west, 65·29 in Sandgate, 52·24 in Undercliff, 28· in Hastings; of north-west, 56·35 in Sandgate, 30·95 in the Undercliff, 12· in Hastings." (p. 21.)

In respect of temperature, Mr. Moseley asserts that the annual mean of Sandgate is high (62·14), and that the alternations are gradual. The mean of the seasons is as follows:

Winter.	Spring.	Summer.	Autumn.
41·76	49·50	61·30	56

We subjoin Dr. Martin's table of the temperature of the Undercliff, Isle of Wight; and of Hastings, from Dr. Mackness:

Place.	Winter.	Spring.	Summer.	Autumn.
Undercliff	41·80	49·82	61·31	53·95
Hastings	39·13	47·68	60·61	55·15

The mean daily range of the thermometer at Sandgate is not considerable; according to our author, it is indeed not much more than in Madeira, and in some months is even less. The mean height of the barometer is 29·85; the number of days on which rain or snow falls in the course of the year is 154, the mean annual quantity 30·50 inches. At Hastings the corresponding numbers are 155·43 degs. and 31·94 inches. The humidity of the atmosphere is less than that of the Undercliff.

After this exposition Mr. Moseley has a chapter on the healthiness of the district, which appears to be satisfactory, and thus he considers that he has made the practical affirmation of the proposition that benefit might be expected from the favourable position and climate of Sandgate. We have brought these statements as we find them before our readers, for we possess no data by which to test Mr. Moseley's facts. His book appears to be written truthfully, and without exaggeration. We see no reason to doubt that he has exercised care in the collection of his facts, and if so, we cannot question that Sandgate may take its place by the side of, or at any rate very near to, those favourite places of resort, Hastings and the Undercliff.

Art. VI.—*An Expository Lexicon, of the terms ancient and modern in Medical and General Science.* By R. G. Mayne, M.D.—*London,* 1853. Part I., pp. 153.

Dr. Mayne has been engaged for many years in the preparation of this lexicon, but the great expense attending the publication of a work of the kind has hitherto delayed its appearance. The present Part is the first of six, and reaches to the beginning of the letter C. We have looked through it, and can safely say that, if Dr. Mayne can carry out the rest of his work in the same way, he will have conferred an advantage on science generally, and especially on medicine.

ART. VII.—*The Microscope, in its special application to Vegetable Anatomy and Physiology.* By DR. HERMANN SCHACHT. Translated by FREDERICK CURREY, Esq., M.A.—*London*, 1853. 8vo, pp. 131.

MR. CURREY appears to have translated his author faithfully, and has given to the English public a book which will be very useful to all who are learning how to apply the microscope in botanical researches. A work in a similar scale and form, showing the mode of investigation in animal, and especially in human, anatomy and physiology, would be more useful to medical men; but yet much information in the manipulatory part of the process may be learnt by the medical student even from this volume on vegetable micrology.

ART. VIII.—*Medical Reform, being the Sketch of a Plan for a National Institute of Medicine.* By AZYGOS.—*London*, 1853. Pamphlet, pp. 77.

THE worst of Azygos is, that he is too good. Give him a clear field, a *tabula rasa*, on which his genius for organization may have fair play, and a full-grown national institute, with "scientific, educational, practical, financial, political, and miscellaneous departments!" would appear, and beneath it would range, in perfect order, all the affairs of hospitals, dispensaries, colleges, and poor-law boards. The objection to the plan is, that it is totally impracticable; it is the dream of a man who has a clear sense of what *should be*, but who ignores, without the least difficulty, everything *that is*. In this pamphlet is a great deal of sense; there are numerous observations which no one will gainsay, and many suggestions which might be adopted with advantage. But its plan commences at the wrong end. Medical reform must come by the re-adjustment and mutual blending of existing institutions, and not by universal destruction and a new creation. We are afraid that, like the constitutions of Sieyes, or of Bentham, the plan of Azygos wont work.

ART IX.—*A Treatise on the Venereal Disease, by* JOHN HUNTER, *with copious additions by* DR. PHILIP RICORD. *Edited, with notes, by* F. J. BUMSTEAD, M.D.—*Philadelphia*, 1853. 8vo, pp. 520.

DR. BUMSTEAD has reprinted Dr. Babington's edition of the treatise on the venereal disease, and has added the notes of Ricord which are contained in the second French translation of Hunter's great work. A few notes are also added by himself, or are copied from Babington. He has thus made an extremely useful work. We have all of us wished to know what Ricord has to say on various Hunterian doctrines, but as no one wishes to read Hunter except in his native, idiomatic, and expressive dress, we can better hear Ricord in English, than Hunter in French. The language of the disciple is justly subservient to that of the master.

We have lately considered Ricord's opinions at length, and need not now recur to them. Those who wish to see them shortly stated and clearly brought out, as derived from, or as modifying, Hunter's doctrines, cannot do better than consult Dr. Bumstead's edition.

ART. X.—*Transactions of the Pathological Society of London.* Vol. IV. *London.* 8vo, 1853. pp. 286.

WE have expressed on several occasions our sense of the value of the successive volumes issued by the Pathological Society. Our own pages, indeed, have shown that the facts recorded in them can be turned to good account. The present volume is, we think, superior even to its predecessors, and contains many instructive cases and commentaries, among which we may especially mention Dr. Peacock's account of the mode of closure of the foramen ovale, and Dr. Handfield Jones' highly important cases of degeneration of the mucous membrane of the stomach. On this subject, we wait with anxiety for extended and independent observation. Several beautiful illustrations are introduced, and the arrangement of the subject, and the general getting-up of the volume, leave nothing to be desired.

ART. XI.—*Summary of New Publications.*

IN addition to the works reviewed or noticed in previous pages, we have received a number of other treatises, which we propose at present merely to enumerate. We shall have occasion to return to certain of them more at length.

In *Medicine*, there are numerous new books. We may notice first, the commencement of an elaborate work on 'Pathology and Therapeutics,' edited by Virchow,* who is assisted by fourteen other physicians, among whom is Vogel (who undertakes kidney-disease), Lebert (who will treat of disease of the blood and lymphatic system), Hasse (who will write on diseases of the nervous system, &c.) Hebra (who will discuss the cutaneous affections), and others equally eminent. The first half of the first volume has alone appeared; it is entirely contributed by Virchow, and treats of the common forms of disease, of fever, inflammation, congestion, dropsy, &c. The work will be one of great extent, and will, doubtless, be of extreme value and repute.

Dr. Guy has brought out a fourth edition of Hooper's 'Physician's Vade Mecum,' a useful work, too well known to require comment from us.

Mr. Parkin has published a short pamphlet on the 'Remote Cause of Epidemic Diseases,' which is a continuation of a large work on the same subject. Mr. Parkin has investigated the course of cholera and yellow fever with great assiduity, and seeks to connect these diseases with volcanic action. This is a curious hypothesis, which has been worked out fully by Noah Webster, and others, but which is still unsupported by any convincing evidence. Mr. Parkin's former work, and the present pamphlet, are, however, well worthy of perusal.

Mr. Finlay has written a short paper on the 'Remittent Yellow Fever of the West Indies,' which we shall consider with other works on the same subject.

As might be expected, treatises on 'Cholera' are becoming numerous. Dr. Scott Alison, Dr. Hearne, Dr. Gilkrest, and Mr. Barwell, are the authors of the smaller ones before us. Dr. Hearne speaks in very strong terms of the utility of the administration of acetate of lead (three grains) and opium (1 grain) every fifteen minutes, in the diarrhœal stage. The review of these treatises will be best deferred till the close of the epidemic.

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* Handbuch der speciellen Pathologie und Therapie. Erlangen, 1854.

Dr. Lindwurm has published a treatise on the 'Typhus in Ireland,'[*] in which the opinions of Jenner on the non-identity of typhus and typhoid fevers are assailed. Dr. Lindwurm's conclusions are not, however, supported by his facts, and after a careful perusal of his work, we cannot congratulate him on the accuracy of his deductions, although we willingly admit the faithfulness with which his facts were collected. We shall enter more fully into detail hereafter; at present we content ourselves with directing the attention of those interested in the point to this rejoinder of the "single-fever" school.

A useful treatise on 'Small-pox,'[†] with much historical information, from the pen of Dr. Eimer, will well repay perusal. The committee of the Epidemiological Society, who contemplate, it is said, an extended treatise on this subject, will find much to interest them in this little work.

'The Blood, in its Diseased Conditions,'[‡] is the title of a work by Dr. Kehrer, from which we expected great things, but which has disappointed us. We shall extract what there is of novelty in it when Lebert's article on the blood, in Virchow's new work, reaches us.

Dr. Bennett's late treatise on 'Tuberculosis,'[§] will, of course, be read by every one. The object of the book is twofold; first, to vindicate the author's claim to the credit of having introduced the use of cod-liver oil into this country, and secondly, to state his opinion in detail on the tuberculous and allied conditions, and to give evidence of the use of the oil. As to the first point, there can be no doubt that Dr. Bennett is entitled to the credit he demands, as, most certainly, he first made known the great powers of this now so universally used remedy. With respect to his views on tubercle and tuberculosis, there will, doubtless, be great differences of opinion, and we doubt whether Dr. Bennett can maintain some of his positions. No one can read the work, however, without profit, and if, on a future occasion, we may have to express dissent in some cases, in a much larger number we shall only have to express decided adhesion to Dr. Bennett's views.

Dr. Theophilus Thompson has just issued a reprint of his useful and practical 'Clinical Lectures on Consumption,' which we shall review with Dr. Bennett's Treatise.

Dr. Stokes's long-expected work on the 'Diseases of the Heart,' has just appeared, but too late for review in our present number. It is scarcely necessary to say that it is a most valuable work, although in some points Dr. Stokes speaks less confidently of the diagnosis of heart-disease than we should have expected.

A work on 'Rheumatism and Gout'[||] has been published by Dr. Wiss. It contains nothing novel, and has many omissions. The hot-air bath is very strongly recommended in both affections.

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[*] Der Typhus in Ireland, Beobachtet im Sommer, 1852, von Dr. J. Lindwurm.

[†] Die Blattern Krankheit, &c., von Dr. C. Eimer. Leipzig, 1853.

[‡] Das Blut in seinen krankhaften Verhältnissen, von Dr. F. Kehrer Giessen, 1853.

[§] The Pathology and Treatment of Tuberculosis, by John H. Bennett, M.D., F.R.S.E.

[||] Ueber Rheumatismus und Gicht, von Dr. Wiss. Berlin, 1853.

On 'Dysentery,' two pamphlets have reached us; one from Dr. Macpherson, and one (a translation) from Dr. Canta, of Calcutta. Dr. Macpherson's paper is an answer to a recent Report of the Bengal Medical Board, on Mr. Hare's treatment of dysentery by the long tube, by injections, and by quinine. The general Board came to a very decided opinion in favour of this mode, and Dr. Macpherson endeavours to show that this opinion is ill-founded. The arguments he adduces are certainly very strong, and have shaken our faith in the accuracy of Mr. Hare's observations.

A work on 'Paralytic Insanity and General Paralysis' has been published by Dr. Fabret.* Like most French treatises, it is extremely verbose, and although it gives a good summary of the subject, it contains nothing novel. Still it may be useful to those who are occupied with these affections.

A great work has been published by Wedl on 'Pathological Histology,'† which we shall review at length as soon as we can find room.

In *Surgery* the press has been less prolific. Professor Erichsen's comprehensive 'Science and Art of Surgery' will be reviewed at length in our next issue. Professor Syme's treatise on the 'Diseases of the Rectum' has reached a third edition, and is now a well known work. The third edition of the 'Principles of Surgery,' by Professor Miller, has likewise appeared, and is a work of established reputation. Mr. Druitt has issued a sixth edition of his excellent 'Surgeon's Vade Mecum,' a work which we do not hesitate to call one of the best of its class. In matter, in arrangement, and in style, it is equally commendable.

Mr. Wilde's 'Aural Surgery' requires a separate review. It is a great work, and will become a standard authority.

In *Midwifery* no special works have reached us except an American work,‡ which is somewhat quaintly but justly termed by its author a "skeleton collection of facts." There is nothing novel about either the facts or their arrangement, but the plan is good and the work is an useful one.

In *Materia Medica* the work of Dr. Pereira has been completed by Drs. Taylor and Owen Rees. In the preface the editors observe that as the author is no more, they feel themselves at liberty to state that the work, "in copiousness of details, in extent, variety, and accuracy of information, and in a lucid explanation of difficult and recondite subjects, surpasses all other works on Materia Medica." No one can hesitate to endorse this judgment, and we may add that the last 600 pages, which have been revised by the editors, merit an equal eulogium.

The second edition of Dr. Royle's 'Manual of Materia Medica' has appeared. It is much improved, and bears the marks of careful revision Mr. Headland has assisted Dr. Royle in its preparation, and the work is most creditable to both its authors.

Mr. Darby has translated Dr. Wittstein's 'Pharmaceutical Chemistry.' It is a very useful work, and should be possessed by every working pharmaceutist.

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* Récherches sur la Folie Paralytique, et les diverses Paralysies Générales. Paris, 1853.

† Grundzuge der Pathologischen Histologie, von Dr. Carl Wedl. Wien, 1853.

‡ A Manual of Obstetrics, by Dr. Thomas F. Cock, M.D., Physician to the New York Lying-in Asylum, &c. New York, 1853, pp. 250.

'The Druggist's Handbook,' by Mr. Branston, is one of those books which contain all the little formulæ which a druggist may have to know, from a recipe for cold cream, or for artificial asses' milk, to the mode of preparation of hydrofluoric acid, or phosphoric ether. It is certainly an *omnium gatherum*, but obviously is a useful work for those who want an easy reference for simple and common things. It is a small book The contents are arranged alphabetically and are easy of reference.

A work on 'Quinine,' by Briquet, is reserved for separate review; so also is a treatise by Werber on 'Therapeutics'*

In *Physiology*, another part of Ludwig's profound work has appeared. —Dr Brinton has completed the translation of Valentin's work —Dr. Axmann† has published some important observations on the 'Microscopic Anatomy and on the Physiology of the Nervous System,' which we shall consider at length — Lehmann's great work on 'Physiological Chemistry' has reached another edition A few additions have been made, but the chief alteration consists in the omission or modification of passages which were not formerly in harmony with each other, for, as the volumes of the former edition (the second) were published at successive intervals— about a year elapsing between each volume—the statements in the first volume were sometimes at variance with those of the second and third One of the additions describes the mode in which Lehmann has succeeded in forming "blood-crystals" in quantity, viz, by passing streams of oxygen and carbonic acid through the blood.

Bischoff has written a very interesting work, on 'Urea as a measure of the Metamorphosis of Tissue' We have prepared a review of this, and of the second part of Bidder and Schmidt's work, but want of space has compelled us to defer it to our next number.

An excellent treatise on 'Animal Chemistry' has been published by Heintz ‡ The analytical details are given with extreme care.

Dr Hoffmann has published a translation of the second edition of Liebig's 'Handbook of Organic Analysis' The work is of course entirely technical, and deals merely with the methods employed in analyzing organic substances

Among *miscellaneous subjects*, we may mention Erasmus Wilson's edition of 'Hufeland's Art of Prolonging Life,' a work containing much good sense and useful information Hufeland was a "sanitary philosopher," and as Mr. Wilson justly remarks, "the 'sanitarians' of our own day have added little to his sensible instructions" Besides this, there are some curious speculations in this work, especially as to the duration of life in the animal world, in the fourth and fifth chapters, for which we shall endeavour to find room, in connexion with a review of Dr. Van Oven's late interesting treatise on the 'Decline of Life in Health and Disease.'—Dr. Hartwig's 'Practical Treatise on Sea-bathing' must be characterized by a reversal of the usual reviewing phrase; it contains little information in much space.

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* Specielle Heilmittellehre Erlangen, 1853

† Beiträge zur mik. Anat und Phys des Ganglien Nervensystems, von Dr C Axmann. Berlin, 1853

‡ Lehrbuch der Zoochemie. Von H W Heintz Berlin, 1853

PART THIRD.

Original Communications.

Art. I.

The Blood—its Chemistry, Physiology, and Pathology. By Thomas Williams, M.D. Lond., Extra-Licentiate of the Royal College of Physicians; formerly Demonstrator on Structural Anatomy at Guy's Hospital, and now of Swansea.

(Continued from No. 24, p. 486.)

Truth not seldom lies concealed in recesses approachable only by a single path. The path must first be seized in order that the destination may be reached. From the vantage-ground only of a clearly conceived *principle* of inquiry can the mind blend into a symmetrical edifice the ununited elements of a novel truth. How often have acute and sagacious minds neared, but missed, a grand discovery! The history of science abounds in apt illustrations. To the artist who works with the materials of observational truth, it is not given as to the sculptor, to embody the first conceptions of the intellect at once, in the faultless and finished Apollo. The errors of one generation, dimming the lustre of truth, are eliminated by the next. In these papers the general proposition has been more than once confidently enounced, that in the zoological series the *fluids* of the living organism grow simpler and simpler in chemical and vital composition in proportion as the lower extreme of the scale is approached, and that, correspondently with the fluids, the fixed solids display a constantly increasing tendency to simplification. To reverse the proposition, the free fluids and the fixed solids constituting the mass of the body acquire, in a corresponding ratio as the animal chain is tracked upwards, a greater and greater complexity of composition.*

As respects the *fluids,* this principle has been substantiated by reference to demonstrative facts. This law of progressive complexity on its application to the floating and fixed *solids* remains to be unfolded. The standard of the fluids is raised by an increase in the proportion of albumen, and by the superaddition, at a certain limit in the scale, of fibrine; the standard of solids is raised by the production of new organized constituents

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* In my paper on 'The Fluids,' which was published in the 'Philosophical Transactions' in the year 1851, I ventured to state in general expression the same proposition. At that time, however, I had not through practical research attained to a confident knowledge of the individual facts upon which such a wide-spreading generalization could securely repose. Subsequent investigations enable me to shape a rude prevision into a well-marked principle. I am not at present aware that any approach to the generalized views stated in the text has ever been made in comparative physiology. It is no hasty enthusiasm to predict, that they will confer upon this branch of physiology the character of scientific *constancy* more than any other principle of organization developed by modern research.

within the elementary cells. The relation between the fluids and solids of the living organism is much more intimate, however, and recondite, than that which is implied in this general statement. The blood-proper is the highest form under which the nutrimental fluid occurs in the animal kingdom, but it is not perfect in its composition at its first appearance in the series; it is comparatively simple in its first-born condition: it gradually increases in complexity by a successive increase in the *number* of its ingredients. The *chylaqueous fluid* in the annelids, its superior limit, exhibits a composition much more complex than that which it possesses in the lower radiated animal. In the lowest animal its albumen is least in amount, its floating corpuscles present the lowest features of organization. It may be affirmed as an absolute principle in the chemistry of living beings, that what is not, or never has been, present in the fluids, never can constitute an integral ingredient of the solids.

If *fibrine* forms no part of the fluids of an animal, it cannot exist as a constituent of the solids; it is an absolute organic law that this proximate principle can only be *produced* in the fluids; it is used by the solids only as a building material. Fibrine, properly so called, cannot be manufactured *de novo* out of the elements of albumen by the elementary cells of the fixed solids; these latter cells are capable of no further effort than that of modifying a principle already prepared, into a new and higher organic compound.* Neither albumen nor fibrine exists as such in the interior of any sedentary cells; such situations are occupied only by a principle developed from fibrine or albumen. Below the limit in the zoological scale at which fibrine disappears from the fluids, albumen rapidly falls in relative amount; above this limit both these principles increase in a similar ratio—that is, that animal fluid which contains the largest proportion of albumen contains also the largest amount of fibrine, and conversely, until the latter ceases altogether. A very small proportion of albumen suffices for the production of the simplest order of floating cells; the presence of fibrine is required for the evolution of the highest.

It will be now shown, for the first time in physiology, that the same gradation from simple to complex, from a lower to a higher standard of organization, is traceable in the elementary cells of the fixed, as in those of the floating solids. As the floating solids of the chylaqueous fluid are to the fixed structures of the animals in which this fluid only exists, so are the corpuscles of the true blood to the sedentary solids of those animals in which true blood only exists. Disregarding the floating cells, this new and important physiological law may be thus enounced:—The chylaqueous fluid produces simpler solids than those developed from blood-proper: consequently, the solid structures or organs of those animals in which the chylaqueous fluid constitutes the exclusive medium of nutrition, are more simple than the solids of those animals in which true blood exclusively exists.

Let now the foregoing generalized views be submitted to the test of a concrete examination; let the several systems of the fixed solid structures be traced in their serial evolution; let each system be studied in the characters of its *ultimate cell;* let the *nerve*-cell be first investigated, and in connexion with it the apparatus of the special senses; let then the *muscle*-cell be followed throughout the zoological series; let this new path in organic science be opened by the enunciation of the first grand fact, *that the blood-proper system, and therefore fibrine, the striated muscle-system, and the nervous system, with its associated apparatus of special senses, first appear in the animal kingdom at one and the same limit—viz., at the echinodermata!* Let the zoochemist reflect for a moment on the grounds whereon this generalization is based. Can muscular tissue, in its higher striated phase, exist in the solids without fibrine in the fluids of the organism? If muscle-fibrine is not absolutely in every minute particular identical with blood-fibrine (Liebig), the comparative histology of muscle-tissue will hereafter place it beyond doubt that the latter in its higher variety is derived from the former. *In its inferior phases it is developed from albumen.* Can any physiologist for an instant withhold assent to the self-evident proposition that where there is no nervous system there can exist no special senses? But let the facts be stated in a less extreme form. No comparative anatomist has ever yet indubitably demonstrated a nervous system below the echinodermata; why? because it has never been looked for in the right way. Below this limit, this system does not exist under the same characters with those by which it is so readily distinguished above this standard. The nerve-cell, like the muscle-cell, the floating cell, and the fluids, here begin in a marked manner *to simplify as they descend the scale.* Their characters severally are no longer the same. Like the fluids, or rather coincidentally with the fluids, they change. In order to the pursuit of the inquiry, the fact of this altered physical character in the elementary cells must be first known. But why should these extraordinary events occur at this particular limit? Because here, if traced upwards, the true blood-system, and therefore fibrine, begins; if traced downwards, ends in the animate chain. What can be the signification of this remarkable relation, hitherto completely unrecognised in physiology, between the nerve and the blood-system? Wherefore this agreement of origin, this parity of development? Both systems (*in their ordinary characters*) are least developed in the star-fish, both most evolved in man. Abundant evidence drawn from actual dissection justifies the inference that the presence of the nervous element in the organism implies that of the blood-proper, and conversely. From such evidence it is certain that the lower forms of the chylaqueous fluid are deficient in those elements which are essential to the production of nerve-matter. Below the echinodermata, therefore—that is, in the medusæ and zoophytes, in which no trace of blood-system, but only of the chylaqueous fluid, exists—there can obtain no nervous system, as commonly understood in comparative anatomy; the nerve-cell becomes more and more devoid of contents, as above this point in the scale it becomes more and more pregnant.

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* In a former paper, I stated that in the lowest animal forms, the cells of the *fixed* solids might generate those compounds which, in the example of the higher animals, were produced by the *free* cells of the fluids. More recently, an extended series of observations has assured me that this statement can be accepted only in a qualified sense—that the fixed solids owe their character to the constituents of the fluids in a much more complete and intimate manner than has ever yet been supposed by physiologists.

As the chylaqueous fluid is to the true blood, so is the source of current power below the echinodermata to the *ordinary* nervous system. This "unknown quantity" in organization is yet undiscovered. It must exist, and science will at no distant time define its nature. It will exhibit the same relation to the chylaqueous fluid with that which the higher nervous system displays to the true blood. To affirm, with some physiologists, that in the zoophytes and acalephs, *the* nervous system exists under a diffused form, were to prejudice this novel inquiry by the employment of the vague nomenclature of gratuitous conjecture. It must be as unlike the cerebro-spinal power as the true blood is dissimilar from the chylaqueous fluid. Two fluids so diverse in vital standard and chemical composition cannot evolve identical products. The highly pregnant cells of the grey nerve-matter in the brain of the mammal could not be developed from such incomplex fluids as those of the star-fish.*

The relation of mutual dependence sought here to be established between the nervous and the blood-proper systems, is most instructively exemplified in the fact of the parallelism presented in evolution of the brain or cephalic ganglia and the heart, the *centres* of these two systems respectively. The cephalic ganglia first appear where a central propulsive power for the fluids first appears—viz., in the crustacea in the articulated series. How inferior the standard of the brain in the fish, how degraded in the reptile, how exalted in the mammal! How many groundless speculations in science would not the clear apprehension of this simple principle of organization have saved. What philosophical anatomist will *now* delve into the maze of the lining structures in vain search for indications denoting the presence of special senses in those classes of animals in which the conditions (as respects the fluids) essential to the existence of a centralized nervous system are wanting? It will be afterwards shown that M. Quaterfages, in describing the eyes of the annelida, has fancifully constructed optical mechanisms which could by possibility have existed in presence only of those organic conditions which obtain in the highest animals. If special senses exist in those classes of animals which are situated on the scale below the limits of the *ordinary* nervous and the blood-proper systems, they must necessarily present characters simplified in a corresponding ratio to that distinguishing the elements of the solid parts of the organism in the same classes; they must see without eyes, and hear without ears. At no distant period, however, more extended and philosophical views will *direct* the researches of science in the resolution of these recondite points in vital dynamics.

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* It is a remarkable circumstance, that a most earnest and extended study into the comparative histology of the elementary cells of the solids and fluids throughout the animal kingdom, should have led me at the same time, but independently, to conclusions on the subject of the "cell-theory" directly opposed to the views so masterly developed by Mr. Huxley in the last number of this Review (October 1st). To me it has constantly appeared, that as the eye of the observer traces upwards the cell of the same *system of solids*, it becomes more and more laden with contents. It is the "endoplast" of Mr. Huxley, in diametric variance to the tenour of his own argument, which *really* increases. The cell-wall, *his* "periplast," as the scale is ascended, becomes evidently more and more subordinate. This element of the cell may indeed project into angles, or alter its contour in other ways; but this is not the symbol of *increasing development*. The purpose answered by the angular elongations of the cell-wall, in the example of the elements of cellular tissue, is merely mechanical—it is connective and subordinate. It is the *grey contents* of the ganglionic nerve-cell which disappear as the scale is traced downwards, not the *cell-wall*: followed upwards, it is *not* the cell-wall (*his* "periplast") which exhibits signs of increasing development—it is the cell-contents, *his* "endoplast." Nerve-matter exists in the acalephs and zoophytes under the simple character of a cell-wall, without any other contents than a homogeneous fluid.

The same observations precisely will apply to the instance muscle-cell. It is the *cell-wall* which first appears in an identifiable form in the animal series. The cell-contents, as the observer mounts upwards in the chain, assume a palpably organized aspect. The *striæ* appear. The *nucleus* acquires a character of increasing importance. These *facts* are utterly irreconcilable with the theory of Mr. Huxley. If true of any form of elementary tissue, his views can only apply to the cells of *areolar* tissue. In this instance, the cell-wall, *his* "periplast," does seem indeed to perform a function, and to assume physical characters superior to those of the cell-contents, *his* "endoplast."

The comparative histology of the muscle-cell teaches a lesson of extreme interest and value in these inquiries; the study of this tissue will hereafter afford material aid in unriddling the enigma of the origin, uses, and destination of the floating solids of the nutritional fluids. On a future occasion detailed reference will be made to the researches of Mr. Bowman and M. Lebert upon this branch of histology. Neither of these distinguished observers has seized the clue to the real law by which the development of muscle-cell in the zoological scale is governed. There can be no doubt, that in the instance of this tissue as in that of other elements of the solids, the *principle of embryonic development* is strictly analogous to the zoological—that is, as the fluids grow in complexity of composition with the growth of the embryo, so the elementary cells of the solids rise in the scale of organization. The author has verified this principle in the most satisfactory manner with respect to the floating cells of the embryonic fluids. They exhibit gradations which distinctly correspond with the several steps in the animal series; there is a remarkable agreement between the embryonic and zoological laws; future science will prove this to be more intimate than it is now conceived. The *striation* of the muscle-cell begins at the crustacea; this character is lost below this limit; but the muscle-cell does not disappear; it exists under a simpler construction; it consists of a cell of variable shape, filled with a homogeneous albuminous fluid. The muscle-cell in the acalephs and zoophytes has never yet been clearly traced; it will be subsequently described. In these lowly forms it is the *cell-wall only* that exists. The *irritability* of this cell is *inversely* as its relative position in the scale—its *contractility directly*. The characters of the muscle-cell in its rudimentary or simplest form, prove with great clearness that *irritability* is the primary property of this cell, and it indwells in the *cell-wall.* Fluid is incapable of altered bulk. The contents of this cell, therefore, in its first condition, being simple fluid, cannot minister to the dynamics of the organule; *contractility* is a quality which is superadded to the former at the highest stages of its growth. The *contents* assume in this comparative view an unquestionably higher organic title than the cell-wall; but the latter is not an inert element, it is endowed with active properties; they persist after the departure of the former. The completeness of the former marks the maturity of the cell. The cell-wall is comparatively perfect, even at its first appearance in the zoological series. It is evident, therefore, that irritability is a lower order of power than contractility; the former inheres in the muscle-cell, and is independent of the nervous system; the latter is superadded coincidentally with the occurrence of nerve-tissue in its higher form in the organism, and with the evolution of fibrine in the fluids. It is the product of the reagency of nerve-power upon muscle-cell in the higher conditions of the latter. *Voluntary* muscular action, therefore, presupposes a certain organization in the muscle-cell; under the circumstances of the *simplest* form of the latter, this mode of action is not possible. In tracing the muscle-cell upwards in the animal series, it is not the *cell-wall* which acquires greater and greater complexity, it is the nucleus and the protoplasm;[*] not the sarcolemma, but the nucleus. The growth and increment of living matter does not occur in the cell-wall; this is stationary; these vital movements occur endogenously not exogenously, centripetally not centrifugally. To the cell-wall, therefore, belongs unquestionably a nutritive, formative, productive value, as well as a dynamical. Comparative histology *proves* it to exist *anteriorly* to the contents; the latter must, therefore, owe their existence to the agency of the former.[†]

Thus, only a prophetic outline has been unfolded—views of organization, which are destined to grow in after stages into the importance of the *first principles of physiological science.* They impart to this department of knowledge the character of *constancy.* They invest it with all the fascinations of a *true science.* They claim an exalted rank. They prove it to be capable of wide-extending generalization. Given the fluids, it is required to judge of the zoological rank of the solids. Science smiles with joy at the captivating newness of the problem. The *unity of organization* is no longer a phrase; it is a substantive reality; it is a demonstrated principle. It is not a vision of an enraptured brain, but a veritable law. *One idea* pervades all animate nature. It is legible in the solids as in the fluids. Both obey the same mysterious impulse of evolution. Both march in parity of growth. Both acknowledge the same governance. The clear apprehension of generalizations is at once the triumph and the summit of science.

This apparent digression is not chargeable with irrelevancy. It elucidates the chief question now being considered. If the fixed solids in those classes of invertebrata in which the chylaqueous system only exists, differ so signally from the *fixed* solids which accompany the blood-proper system, it is certain that the *free,* floating solids of the fluids in these classes respectively, must be separated by no less marked differences.

Echinodermata.—In the zoophytes and medusæ no *cavity* exists *external* to the digestive apparatus. Everything is solid. The stomach is multiplied into diverticula. These diverticula are equivalent and homologous to the splanchnic chamber; they contain a corpusculated fluid. It is the only and exclusive ministering agent of solid nutrition. This is unmixed "*Phlebenterism*"[‡]—that is, the stomach opens directly into those canals which represent the blood-system—gastro-vascular. The word *Phlebenterism* is strictly applicable to the digestive and vascular system of no other class of animals than the zoophytes and acalephæ. Here the digestive diverticula are charged with the true nutrimental fluid—they are "Veins." In the echinodermata these parts are no longer the homologues of vessels. They are filled with chyme, not nutritive fluid in its matured form. The latter is lodged in a *separate* and *closed* chamber. In this class, for the first time in the animal series, the immediate products of digestion are divided off from the true nutritious fluids. The peritoneal cavity, destined in the vertebrated animals to prevent friction between the abdominal viscera and the parietes, in the lower invertebrata becomes a reservoir of nutritional fluid. It is the normal anatomical place of the chylaqueous fluid. In no single example above the echinodermata does it *directly* communicate either internally with the digestive organs, or externally with the surrounding elements. Its fluid contents are derived from that of the digestive cæca. It is an independent system. In this class it is the grand agent of nutrition. Though holding in solution only a small proportion of albumen, and scantily corpusculated, its nutritive capacity admits of conclusive demonstration. The inferior echinoderms, the Asteriadæ and Echinidæ, are remarkable for the passive inertness of their habits. Distinguished for the sluggishness of their muscular power, they move by indirect mechanical provisions. Delle Chiaije, Tiedemann, Sharpey, and Dr. Grant describe a *nervous system* in this class. The author has instituted numerous dissections in search of this system. He is persuaded that, like the blood-proper system in the Asteriadæ, Ophiuridæ, and Ophiocomidæ, Tiedemann's description exaggerates its true and real proportions. The nervous, blood-proper, and muscular systems in these inferior genera of echinoderms, present *equally* the same character of *incipiency.* This fact is one of extreme interest. It will be presently shown to be destined to reflect bright light on the question which respects the character and origin of the floating solids. If the echinoderms were endowed with a high degree of muscular activity, it might be predicated with certainty that they were also gifted with a correspondingly developed vascular and nervous systems, and the opposite. One system could not, *by its very laws,* exist without superinducing the others. If the scalpel of the zootomist demonstrates the presence of a clearly defined *nervous* system, the physiologist, enlightened by a right apprehension of the principles herein advocated, may infer with confidence that a true-blood system must also exist. This is *science* in its most exalted state. Such method of reasoning is uplifted immeasurably above empiricism. It clothes chaos in the attractive apparel of certainty.

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[*] I would suggest the use of the word *mesoplasm* to distinguish that compound which occupies in all cells the space between the nucleus and cell-capsule. The word *protoplasm* introduced by Schleiden involves *a theory.* The term *mesoplasm* simply affirms locality.

[†] The argument developed in the text is not the offshoot of a reckless speculative fancy. It is the justified growth of very extended and laborious research. I undertake to substantiate the propositions stated in the text only in general terms, by subsequent reference to facts, details, and illustrations drawn from actual nature. I am very deeply imbued with the belief that these views, when elaborated into the ripeness of full and finished demonstration, will constitute hereafter grounds in physiological science, whereon to rest wider and grander induction than has ever yet been attempted.

[‡] At a future stage of these studies, while discussing the fluid systems of the mollusca, I hope to set at rest the controversy between M. Quatrefages and Messrs. Alder and Hancock, on the subject of '*Phlebenterism.*' At present, I will only venture to state, that neither of these distinguished controversialists has clearly apprehended the deep meaning which lies concealed beneath the exterior of this term. The English naturalists are undoubtedly further removed from the truth than the French *savant.* I claim the privilege only, in this place, to declare, that at least for twelve months before the memoirs of M. Quaterfages had come to my notice, my views with respect to the structure and uses of the cæcal diverticula of the alimentary system in the entozoa, annelida, and mollusca had been matured and published. (See Report on the British Annelida, Trans. of Brit. Association, 1851, and Paper in Phil. Trans., in 1852.)

In the echinoderms, possessing the blood-proper and nervous systems only in their most indistinct and rudimentary conditions, the *organs of special sensation* are not physiologically possible. An apparatus of special sense supposes a complex evolution of some part of the periphery of the nervous system, if not also of the vascular. Can the circumference of a system antecede the development of the centre? Such supposition is opposed to every canon, everything that is *certain* in organic science. The ocelliform spots observed by Professor E. Forbes at the extremities of the rays of certain species of Stelleridæ, and supposed by this naturalist to be connected with the extremities of nerve-filaments, and described as protected by a peculiar arrangement of minute spines around each, are really nothing but pigment points. They are not constant in different individuals of the same species. They do not approach in minute structure to that of true ocelli. There lies beneath them, or in connexion with them, no concentration of nerve-matter. They are remote from the real centres of the nervous and blood systems. They possess not one character distinctive of an optical instrument. The microscope proves what *à priori* principle demands—that they are not eyes.

The echinoderms may be emphatically distinguished as the "region of cilia." *In them muscularity is transmuted into ciliarity.* As the muscle-cell surrenders its contractility it acquires an extra proportion of irritability. What it loses in the one property it gains in the other. This fact offers a significant clue to the discovery of a new law in biological science —that *ciliarity* is really only that property in an epithelial cell, which in the muscle-cell is denominated *irritability.**

In these torpid, motionless animals, ciliary replaces muscular power. The arms of the Ophiuridæ display an exquisite degree of apparent sensibility—that is, they writhe and contract for some time upon the contact of the minutest object. And yet, in the soft structures of these sensitive parts, it is impossible to detect any other element than the smooth, *irritable* cylindrical cells, so abundant in the soft structures of the actiniform zoophytes. These cells offer no approach to *striæ*. *Striation* in muscular fibre expresses by its degrees the measure of *voluntary* power. The doctrine which contends for the interchangeableness of muscular and ciliary power, receives support from another and most extraordinary order of evidence. In insects and crustacea a true ciliated epithelium does not occur. *These are the very classes in which the muscle-cell first acquires the striated character!* The suppression of the *ciliary* is marked by an augmented development of the *muscular* system. Is not this signal coincidence a demonstrative proof of the convertibility, of the correlativeness of these two great molecular forces? The word "voluntary" implies a *centre* of volition. In the radiata such centre has no existence. If this, the highest order of power, has been denied to the radiated and annulose animals, the *striated* muscle, its executive instrument, *cannot* have been provided.

Thus through phenomena the mind mounts to the apprehension of principles—it rises from matter to "force."

Among the Asteriadæ and Echinidæ the author has instituted numerous and rigorously exact dissections, with a view to define the character of the

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* The numerous and varied researches which, during the last ten years, I have prosecuted into the organization of the invertebrated animals, have amassed in my mind abundant evidence tending to this conclusion. The muscle-cell of the actinia is highly irritable. This property inheres in the *cell-wall.* The cavity of the cell is filled only with a non-granular fluid plasma, in which it is impossible that any such property can reside. In these animals, of whose ultimate structural elements irritability is so marked a characteristic, the *ciliated cell is capable of altering the form of its outline*—that is, the *membrane* of the cell-wall, as well as the cilia, which are substantively its continuations, *contracts* and *dilates*, like the involucrum of the muscle-cell. This extraordinary fact cannot be observed in a single detached ciliated cell; but in *an adherent group* it admits of convincing demonstration. This plain principle divests ciliarity of its mystery; it is no *new* power; it is muscularity seated on an unexpected element. It is the normal property of the lowest form of muscle-cell manifested by a differently configurated organule. Kölliker's discovery will hereafter extend its range from the deep regions of the skin to the superficies—from the dermis to the epidermis.

nervous system. He is *certain* that grey nerve-matter, the ganglionic, has no place in the organization of these genera of echinoderms. The *low* character of the muscle-cell, the incomplete condition of the blood-proper system, impress his mind deeply with the belief that in such simply constructed organisms, ganglionic centres, grey nerve-matter, can have *no purpose* to *answer*. The blood-system here does not *circulate* its contents. The parietes of the vessels are not endowed with a power of *contracting* upon the contained fluid. This fact admits of ready and unquestionable proof in the conspicuous bloodvessel of the *Sipunculus*. The first business of the nervous system is to confer upon the true-blood system the power to circulate its fluid contents. Fluid cannot move of itself; its motion always is due to an applied force. The absence of this circulating power involves necessarily the absence of a *centralized* nervous system. Thus theory reaches the eminence of a new truth, the practical demonstration of which may remain to distinguish a future epoch in anatomical science.

Innervation is executed in these inferior beings through the instrumentality of an apparatus, the characters of which are not yet known. Analogy drawn from the demonstrated descensive simplification which occurs in other systems of organs, convinces the author that a corresponding simplification *must* take place in that of the nerves.[*]

Could the brain of a mammal be sustained materially or dynamically by the fluids of an echinoderm? This is an extreme interrogatory; but it imparts cogency to the argument. It gives substance and validity to the principle which demands that the fluids and the solids should stand in a *direct* ratio to each other.[†]

The simple nerve-cell of the radiata and zoophytes will hereafter be proved to bear the same relation to that of the higher animals with that which the muscle-cell of the former does to that of the latter.

The fluids of these classes respectively will exhibit a similar relation.

In organisms of which the solids are so simple[‡] the fluids must exhibit a *correlative* simplicity. Thus a *new* and powerful argument is developed by the study of the solids, which lends its force to sustain the conclusions derived from the immediate examination of the fluids. The physiological capacities of the fluids cannot be satisfactorily proved, so long as the fluids only are subjected to analysis. The results of this analysis must be compared with those obtained by a similar investigation of the solids. A comparative *chemical* analysis of these two grand constituents of living beings rewards the labourer far less satisfactorily than a *physiological*. To prove that both contain albumen, or that both contain certain salts in common, &c., is labour fruitless of good. It accomplishes more for science to demonstrate that, in *all animals*, the liquid *un*organized and the solid organized moieties of the living body, are *invariably* and *necessarily* linked into unity by an intimate agreement in general properties. The simple fluid produces a simple solid, the complex a complex. This method of investigation will afford novel aid in unravelling the tangled knot of the rise, function, and decline of the floating cells of the fluids. These latter must stand in the same relation to the fluids as the organized fixed solids. Both are derived from the same source. If *fibrine* does not exist, nor ever has existed, in the fluids, it is quite certain that in neither the locomotive nor sedentary solids *can* it be present, unless it be manufactured *de novo* out of some other element by the latter. Such an idea is as yet wholly gratuitous and untenable. It is supported by no single ascertained fact. The fluids are the scene wherein are *prepared, produced*, the organic principles engaged in the fabrication of the solids. True fibrine and albumen exist nowhere but in the nutritive *fluids*. By the muscle-cell the former is modified only into musculine; by the areolar and cartilage cells the latter is *modified* only into gelatine, &c. Zoochemistry is indeed at present little acquainted with those liquid principles which are contained in the interior of the cells of the sedentary solids. Organic chemists have long supposed that a certain class of solids can be derived from a certain correlative class of the constituents of the fluids. By the late Dr. Prout this argument was prosecuted with great ability into the domain of pathology. Upon its basis his genius raised a new theory of therapeutics. Comparative histology lends to this ancient doctrine an unexpected sanction. It is *quite certain* that those elements in the organized solids of the higher animals which do not exist in the organized solids of the lower, must be derivatives of those elements in the fluids of the former which are not present in those of the latter.

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[*] On a future occasion, while discussing the *unity* of the laws by which the fixed and floating solids are governed, I will verify the statements contained in the text, by the testimony and authority of illustrations drawn with the utmost care from nature.

[†] I may be pardoned here for digressing so tangentially into a subject which, however, grows most naturally out of the chemico-vital question argued in the text. I prophecy, with a feeling profoundly earnest, that when pathological fluids and pathological solids shall be studied in their reciprocal relations, in accordance with the logic of the general arguments which I have endeavoured to pursue in these papers, with reference to the physiological fluids and solids, a distinctive era will be established in pathological inquiries. *Why* is it that a muscle-cell has never been developed from the constituents of a pathological fluid—never demonstrated among the elements of a pathological solid? I am persuaded that the *nerve*-cell of inferior pathological solids, when clearly and comparatively defined, will be found to bear the stamp of organic degradation, to exhibit the same relation to the fluids out of which it was evolved, as the physiological nerve-cell in the same organism does to the physiological fluids. But in the human body, how is the pathological element of the fluids, *if separable*, to be separated? Not by immediate demonstration. A clear apprehension of the abnormity of the fluids can only be inferentially reached through a rigorous study of the visible, measurable, demonstrable constituents of the pathological *solids*. Not the *cell*-elements only, but the organized pathological systems, the vessels, and the nerves. These are hints diffidently projected as incentives to acuter minds and more experienced pathologists. No one at present can say what *sort* of pathological solid will accrue from a *given* description of pathological fluid. Nor until this refined summit is attained can pathology rightly claim the honoured distinction of *a science*. *Pathology* is now really nothing but a vast wilderness of *un*united facts. The quickening *principle of order* and classification is wanting. The time, however, is fast approaching when a far-seeing and wide-extending genius will pronounce over this sandy waste the solemn fiat, "*let there be light!*" and there shall be light.

[‡] The words "simple" and ' complex," "simplicity" and "complexity," which occur so

The preceding discussion is not barren of results. It pioneers an easy road to a new country. The science of life and living things must be studied as a whole, not as a part. Nothing in the vital organism can be understood if isolated from the systems within systems of which it is an integer.

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repeatedly in these papers, should be here clearly defined. The word "simple," in the acceptation in which it is employed in the text, is designed to denote an organic substance the elements of which are believed to be less numerous than those of the "complex." The terms "inferior" and "superior," "perfect" and "imperfect," "degraded" and "exalted," are objectionable in their application to natural things. Everything in nature is *perfect in its place*. Nothing is more exalted than another. The "complex" could not fulfil the ends of the "simple." In the place of the "simple," the "complex" would be valueless in the mundane system. The muscle-cell or nerve-cell of the zoophyte is more "simple" than the corresponding structures of the mammal, because the former contains a lower order and a fewer number of elements than the latter.

Having now proved that in the echinodermata the whole apparatus of the fixed solids is remarkable for its "simplicity," let the study be resumed which concerns the *free solids of the fluids.*

In this class of animals the nutritive fluids are first gathered into separate and independent systems. The digestive organs are no longer in open communication with the receptacles of the nutritional media. The latter are enclosed in closed independent chambers. Thus is established, for the first time in the animal kingdom, the distinctness of the chylaqueous system. It is a criterion of advanced development. The zoophytic character is here lost. This fact, estimated apart from all its organic relationships, bears the impress of no significance. But view it in its relative bearings, what does it signify? It marks a new and extraordinary epoch in the *creative* history of organic nature. Coetaneously with it come also into existence, under the same circumstances of rudimentary incipiency, the nervous, the muscle, and the blood-proper systems. This is not a fortuitous concurrence of epicycles; one necessitates the other.

It has already been proved that, although this is the exact limit, in the chain of animal life, at which *four* new systems of organs arise, it is *not* the inferior boundary of that of the floating cells of the fluids. The *phlebenteric* fluids of the zoophytes and acalephæ *corpusculate.* The tendency to corpusculation is co-extensive with the animal fluids themselves. The floating cells must, therefore, envelope some recondite meaning. What is it in the instance of the fluids of the echinodermata? The corpuscles of the chylaqueous fluid in the Asteriadæ and Echinidæ are very distinctive in microscopic characters (pl. 2, figs. 21 to 26). They look like spherules of hard and very minute granules of coagulated albumen; they are scantily distributed only throughout the mass of the fluid; they are remarkable for the *absence* of the oleous principles; they possess neither a detectible nucleus nor involucrum; the constituent granules of each corpuscle feebly adhere together; they are readily diffused into individual molecules. This circumstance argues the absence of *fibrine.* It is the cohesive substance, the cement, which, in the case of the blood-corpuscles of the crustacea, unites the component granules. The proportion of albumen contained in the chylaqueous fluid of the Asteriadæ and Echinidæ is remarkably small. The acids render it only opalescent. No clot is formed by heat. It betrays indeed all the *apparent* characters of inorganic sea-water. For this element it was actually mistaken by Tiedemann, Sharpey, and Müller, and the multitudinous copyists after them have re-enacted the error. The presence of albumen, though in minute quantity relatively to the bulk of the fluid, admits of two modes of demonstration. If the fluid, which in these genera is readily collected to any amount, be first strained through fine linen, in order to separate the corpuscles, the complete freedom of the fluid from the latter being tested by the microscope, heat and nitric acid will throw down a conspicuous cloud of albumen. It requires repeated observations to familiarize the eye with the exact characters of the corpuscles. They may be mistaken for the ciliated cells, which are detached into the fluid from the parietes of the containing cavity. They are *undoubtedly* peculiar to, and formed in, the chylaqueous fluid. They are at once the evidence and the product of the *vital* properties of this fluid. They are constant in the same individuals of the same species, different in different.

A close examination of the behaviour of these living solids, conducts to a new proof of the organic nature of the fluid in which they are formed. When placed in pure sea-water they soon fall to pieces. The fluid insinuates itself between the component granules and separates them. If that contained in the peritoneal cavity were really, as supposed by Tiedemann, *un*living sea-water, it is certain that they would behave in the same way in it as in that drawn directly from the sea. The physiological presence of these corpuscles in the fluid of the peritoneal cavity, is, however, placed beyond all dispute by subjecting the little Ophiocomidæ to examination as transparent objects. The animal, being perfectly fresh and perfectly uninjured, will inject and distend fully the integumentary membranous processes with the fluid of the peritoneal cavity. As it whirls in the little transparent cæcal process, its *corpuscles* may be recognised and defined with perfect clearness.

In Comatula Rosacea, abundant in Langlan Bay, on the coast of Swansea, a second (anal) orifice is added to the alimentary system. This fact does not involve a change in the characters of the chylaqueous fluid. Its corpuscles (pl. 2, fig. 21), correspond with those of the Ophiocomidæ (figs. 22, 23, 24), and Asteriadæ (figs. 25, 26, 27). It occupies the same cavity. As in the case of the latter genera, it is *internally ciliated.* The corpuscles are small in size and spherical in figure. They are destitute of nuclei. The oil-element is microscopically and chemically wanting—ether extracts none. A few transparent filmy cells are intermixed with the proper granulous corpuscles. This description of pellucid empty cell is found in every variety of chylaqueous fluid. Its origin is mechanical and accidental.

One type of cell prevails in the chylaqueous fluid of all the Ophiocomidæ. It is a minute, delicate, granulose, spherical cell (figs. 22, 23, 24). In the larger Asteriadæ these bodies present a slightly augmented development. They are larger than those of the Ophiocomidæ, relatively to the size of the animal. The component granules are larger and denser. Sometimes a slight shining molecule appears in the centre of the cell. It has the apparent character of an oil drop. It cannot be so, for ether dissolves out no trace of oil from a mass of these corpuscles. Those taken from the chylaqueous fluids of Solaster Papposa (fig. 26), Cribella Oculata (fig. 27), conform in every particular to the description just given. Though in the Echinidæ a second opening occurs in the digestive organs, the fluid contained in the great chamber of the shell agrees accurately with the chylaqueous fluid of the Asteriadæ. It is not more highly organized. Its corpuscles are not more numerous. In composition they are not superior to those of the latter orders. They possess no nucleus, nor do they contain oil (fig. 28). The light, bright molecules in the interior are optically produced. In Spatangus Purpureus (fig. 29) they are less crowded with granules.

In passing from the Asteriadæ and Echinidæ to the Sipunculidan genera, a very striking change in the number, form, and structure of the corpuscles of the chylaqueous fluid is remarked. The cell is no longer orbicular; *it is a flattened oval.* The Sipuncle is neither a spheroidal nor stellate animal; it is vermiform and cylindrical. Can the *figure* of the corpuscle of the fluid bear *any* relation to that of the body of the animal? Innumerable facts disprove this conjecture.

In the cell of the chylaqueous fluid of the Sipuncle (figs. 32, 34), the hard points of coagulated albumen, the granules, *disappear*. They are replaced by one, two, or more molecules of an oily character in each cell; these molecules display a faint reddish tint. *This fact should be signalized as the first essay of nature to develop pigment in the interior of floating cells of the fluids.* The chylaqueous fluid of the Sipuncle, viewed in mass, exhibits a marked pinkish hue; it presents the same precise colour as the fluid contained in the bloodvessel. In all the Sipunculidan genera it is very thickly charged with albumen. A dense clot is precipitated by heat and acid. The contents of the corpuscles *fibrillate* on bursting, and a stringiness is exhibited by the precipitate carried mechanically down by the subsiding corpuscles. But the presence of fibrine in the nutritive fluids is rendered inferentially *certain* by the augmented development manifested by the muscle and nerve cells. The cephalic end of the Sipuncle is capable of vigorous voluntary muscular action. It protrudes and withdraws its fringed tentaculated head with great activity. This, then, completes the descriptive account of the corpuscles of the chylaqueous fluid of the echinoderms. Let the inquiry be now instituted as to the *process of corpusculation;* what is the meaning of this tendency? where and whence does it originate? whereunto does it point, in this class of animals? At the echinoderms, as already stated, begins the *closed fluid series.*[*] The latter represents a mixture of chyme and a nutritive fluid, the former of blood and water. The latter is *in part* an *aquiferous* system, the former in part a digestive.[†]

That which the author has recently distinguished as the chylaqueous order of fluids, has for half a century been known to natural observers under the name of the *aquiferous.* The aquiferous system of naturalists supposes an immediate passage of the external water into the cavities of the animal body. The author admits that, into the chambers containing what he has ventured to designate as the chylaqueous system, the external element can only find admission through the digestive and cutaneous parietes—that is, through *imperforate membranes by endosmosis*—for the chamber containing the true chylaqueous fluid is a closed space: it has no *direct* communications with the outward medium. The external element must, then, pass through a *living* partition in order to gain the cavity of the peritoneum. The organic principles can only be derived from the digestive cæca. In these receptacles chymification is accomplished. The author has lately proved, by very clear observations both in the Asteriadæ and Sipunculidæ, that the nutritive fluids, *before* they leave the digestive system, that is, while they are yet *within* the alimentary diverticula, *manifest a very distinct tendency to corpusculation.* This fact is really a crucial demonstration. It is conclusive of one point in the history of floating cells—that they are capable of arising *spontaneously* in the fluids—that is, without the intervening agency of any pre-existing solid, fixed or free. In this case the chyme has not traversed a partition of *living cell-structure*, as in the instance of the contents of the thoracic duct of mammals. It has undergone admixture only with the biliary secretion poured into the digestive cæca by the glandular parietes of the latter. It is the re-agency of this fixed cell-product upon the albuminous fluid resulting from the digestive process, which really and immediately confers upon the latter fluid the *disposition to corpusculate*.

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[*] See the author's paper in the Philosophical Transactions for 1851.

[†] In recently re-examining the corpuscles of the fluids of the acalephs and zoophytes, it appeared to me certain that they were distinguishable from those of the chylaqueous fluid of the echinoderms in two prominent features—1st, that they were absolutely and relatively larger; 2nd, that they contained oily molecules. Such characters are distinctive of the corpuscles of the *open* fluid series, and which in future I propose to denominate the *phlebenteric corpuscles.* Corresponding phlebenteric corpuscles will be afterwards described in the entozoa, annelida, and molluscs.

The cells which thus arise in chyme are *identifiable* beyond question, with those which float in the fluid of the peritoneal cavity. In the Sipunculidæ the former are round (fig. 30), the latter flattened and oval (figs. 31 to 34). The former represent the latter grown only to a certain point. In *this* situation they can attain no greater dimensions. They can acquire the matured and perfect size and figure only in the splanchnic cavity. The chylous corpuscles which appear in the nutritive fluid while the latter is yet *within* the digestive system do not pass bodily through the parietes into the peritoneal cavity—they *first fluidify*. The fluids only traverse this partition. This act of passing from one cavity to another through an interposed membrane, is undoubtedly, in this case, one of simple physical exosmosis, aided by compression. The digestive diverticula of the starfish are lined by only a *single* stratum of liver-cells (consisting of a *closed* hyaline involucrum filled with adipose molecules, which differ in colour in different species), *irritable* areolar filaments, and externally, that is, facing the peritoneal cavity, a layer of ciliated epithelium. There is here no trace whatever of blood-proper vessels—no nerve-threads. The act of passage, then, is really one of physical exosmosis. It is the *tendency to corpusculate*, not the corpuscles themselves, which travels with the fluid from the digestive cæca into the peritoneal cavity. Arrived at the splanchnic cavity, the fluid corpusculates *de novo*. But here the tendency in this direction is intensified; cells more rapidly and numerously arise; they attain to a further limit of growth.

In conclusion, the author is anxious to state one fact, with reference to the corpuscles of the chylaqueous fluid of the echinoderms, more especially of the Sipunculidan orders, which he has *proved* by numerous and most scrupulously exact observations. It is the *cell-wall* of the corpuscle which *first* appears. It arises, unquestionably, in the homogenous fluid—not through the procreative agency of an anterior parental corpuscle, but in the amorphous fluid itself, by a spontaneous act of cellulation. The cell-wall, now a closed vesicle, grows larger and larger by the inbibition of fluid from without. In process of development a minute, highly refractive pinkish molecule* arises at some point (not *necessarily* its mathematical centre) in the interior of the cell. It is the special depository of the pigment. In structure, refractive power, and colour, it is totally unlike the cell-wall. It seems impossible that it could at any time grow into the latter, and thus perpetuate the cell. It never exceeds the dimensions of a mere speck, a molecule, compared with the size of the containing involucrum. If it be only an adipose molecule, and not a physiological nucleus, then these cells *have no nucleus*. Is not the inference uncanonical? not necessarily, for the office of the cell-wall is to *secrete* from the surrounding fluid the contents of the cell. This act of cell-agency results in the production of a plasma more highly vitalized, further modified, than that external to the cell-wall. In the former (*intra*-cellular plasma) a *nucleus* is evolved, in the latter (the *inter*-cellular liquor) a cell-wall only is produced. The former is a higher creative act than the latter. The materials used in the first instance stand above those employed in the last, in the scale of organic principles.

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* Let the reader carefully examine the *gradations* amongst these cells represented in figs. 31, 32, 33, 34 (plate 8), which are drawn from the real objects.

(*To be continued.*)

ART. II.

On Collapse of the Lung and its results, considered in relation to the Diagnosis and Treatment of certain Diseases of the Chest. By W. T. GAIRDNER, M.D., one of the Ordinary Physicians in the Royal Infirmary of Edinburgh.

IN the 'British and Foreign Medico-Chirurgical Review' for April and July, 1853,* as well as in some preceding papers, I have endeavoured to establish, from the point of view of morbid anatomy, certain conclusions, which, if correct, must be admitted to have an important bearing on the diagnosis and treatment of thoracic affections. It is the object of the present article to embody some of those practical ideas, which appear most directly to spring from the pathological doctrines alluded to: and to point out such of the opinions and practices current in the present day as appear either to be founded essentially in error, or to be liable to receive an erroneous bias from imperfect pathological views. To fulfil the first part of this programme would be to write a treatise on thoracic diseases; to do justice to the last would require a complete critical review of the opinions of the present day, as expressed in the works of the most accredited authors. I shall, however, attempt to confine this paper within bounds more suitable to the space assigned to it; and, by seizing the most familiar and clear aspects of the subject, to bring before the reader a few points of great practical importance, free from all unnecessary discussion. The previous elaboration of the preliminary matter will, I trust, prove a sufficient excuse for any appearance of undue dogmatism, or neglect of contemporary statements, in the present article.

The most important conclusions enunciated in the communications referred to above, as the result of recent inquiries into the pathology of the thoracic viscera, may be shortly stated in a few sentences. It is, I think, sufficiently clearly proved, that among the different forms of recent condensation of the pulmonary tissue, by far the most frequent are those depending on simple collapse of the air-cells, sometimes attended by more or less serous effusion, or vascular congestion. These forms of condensation, when not dependent on external compression of the lung, are, as I have endeavoured to prove, intimately related to bronchial obstruction, which may be looked upon as their chief exciting cause; the amount of obstruction in the bronchi necessary to produce collapse of the air-vesicles being in inverse proportion to the strength and vigour of the patient. The dependence of atrophy of the lung, of emphysema, and of dilatation of the heart, upon these forms of condensation, (the first as a direct consequence, the other two by means of the inspiratory expansion of the chest bearing unduly upon the sound and dilatable parts of its viscera,) has also been fully discussed in the papers referred to. It remains to show wherein these doctrines may be expected to modify to any considerable extent the diagnosis and treatment of chest affections.

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* April, p. 454, and July, p. 209. Articles on Collapse of the Lung, Bronchitis, Emphysema, and Dilatation of the Heart.

The diseases which have been considered, since the application of the modern physical diagnosis to the study of the diseases of the chest, as the chief sources of condensation of the lung, or of dull percussion-sound in the thorax, are pneumonia, pleurisy, or hydrothorax, and tubercular phthisis. Every work on physical diagnosis, every course of clinical and systematic instruction, has usually treated in detail of the modifications of percussion-sound due to each of these causes, and of the collateral auscultatory and other phenomena distinguishing pneumonic condensation from pleuritic effusion, and both of these from tubercular disease. On the other hand, the existence of other causes of condensation, and of dull thoracic percussion, than those above-mentioned, has usually been either overlooked, or has occupied so small a space in the field of elementary instruction, as to disappear almost entirely from the view of the student. In particular, the connexion of bronchitis, and of other diseases producing bronchial obstruction, with these phenomena, can scarcely be said to be acknowledged, otherwise than by a few very cursory, and, as it were, unwilling admissions, in the great majority of standard works. This statement might be easily borne out by extracts from the numerous and excellent treatises on chest disease, which are in the hands of practitioners both in this country and the continent; but the reader will readily pardon the omission of references which are familiar to him, if he finds that in his own experience he has usually regarded dull percussion of the chest as being, in the vast majority of cases, a valid distinction between pure bronchitis and most of the other acute and chronic diseases of the thoracic viscera with which it may be confounded or complicated.

From a consideration of the conclusions mentioned above, it will appear that a correct view of thoracic pathology demands a much wider and more general recognition of the existence of pulmonary condensation from bronchial obstruction, than it has yet received. It must, indeed, be admitted that in one department of practice more just ideas have long prevailed among the best-informed pathologists; and that in infantile diseases the origin of pulmonary condensation in simple collapse of the vesicles in a large proportion of cases, has been taught by numerous observers, to whose elaborate researches and correct appreciation of facts I have endeavoured to do justice in a previous paper. Confining my observations at present chiefly to adult pathology, I shall endeavour to show how far the diagnosis and treatment of pneumonia, pleurisy, phthisis pulmonalis, and bronchitis, seem to be liable to serious error from the imperfect knowledge which has prevailed of the forms of condensation dependent on bronchial obstruction.

1. *Pneumonia.*—Not one of all the acute inflammatory diseases has been the subject of so much and so careful observation as pneumonia. It

has been in the present century, as pleuritis was among the later Greeks and Arabians, the *princeps morborum acutorum*, the battle-ground of opposing principles, both in pathology and practice. There is scarcely a great physician from the time of Sydenham to the present day, whose opinions in respect to it have not been repeatedly canvassed in the monographs and systematic treatises of the last few years. By the medical writers of the present century it has received an unexampled amount of discussion. Its symptoms, its physical signs, its causes, its degree of fatality under different circumstances, its varieties of type according to age and epidemic constitution, its relation to all other diseases, its treatment by bloodletting, by tartar-emetic, by stimulants, by ptisans and *extractum graminis;* its behaviour under Brownism, Broussaisism, Rasorism, and every other variety of *ism;* under homœopathy and every other variety of *pathy;* finally, its issue when left systematically to nature with regulated diet and regimen (for none of the *expectant* physicians have ever trusted nature in these latter particulars);—all these subjects have been discussed under every conceivable aspect, and with the aid of every resource that modern learning and ingenuity can supply for the solution of such questions. In particular, it is to be observed that pneumonia, together with typhoid fever, phthisis, pericarditis, rheumatism, and a few other diseases, has occupied for many years past the most prominent position in all our statistical inquiries, and owes this position no doubt to the conviction that it is a pathological condition more easily recognised, more uniform in its character, and therefore less apt to lead to fallacy than most others. It is certain that the public, and even a considerable portion of the medical profession, are often induced to repose with undue security on numerical inquiries, under the impression that the diseases treated of under one name are made up of identical or nearly similar pathological units. This impression, originally propagated in perfect good faith, by men whose object was to avoid fallacy, and who sacrificed every other consideration to the cause of truth, has recently been turned successfully to account by many persons of another description; men self-devoted to the service of quackery, having no other interest in medical science than a personal one, and ready, accordingly, to confound truth and error by the careful selection, the studious suppression, or the indiscriminate adoption of data.

There can be little doubt that the study of morbid anatomy in connexion with physical diagnosis, has very greatly contributed to fix in the public mind the idea of pneumonia as a separate and peculiar pathological unity. Among the ancients, the *peripneumonia* was, as epilepsy, neuralgia, and typhus fever now are, nothing but the name for a series of symptoms or external phenomena, the presence or absence, the severity or mildness of which in individual cases guided the mind of the practitioner in diagnosis, and his hand in action. No doubt speculations existed as to the anatomical seat of this affection. The expectoration, the pain or uneasiness of the chest, the functional disturbance which it created, were all such as to point to the part affected: and the name of the disease, accordingly, bears the impress of a rough anatomical appreciation of its seat. But the great uniformity of the descriptions of this affection, taken in connexion with the considerable diversities of opinion as to its anatomical nature,[*] show that it was by the study of the evident symptoms alone, and not by supposed anatomical differences, that the ancient authorities concurred in giving this disease a position in the nosology, and particularly in distinguishing it from pleurisy.

Nor did this view of the subject cease with the introduction of morbid anatomy as a means of pathological inquiry. The discovery of *hepatization of the lung* dates from an early period in the history of that science, but notwithstanding multiplied investigations in the seventeenth and eighteenth centuries, Morgagni found it impossible to give an exact anatomical description of the peripneumonia as distinguished from pleurisy; and his successors up to the end of the last century were in no better predicament, as we find that the endless inconsistencies of previous nosological writers on this point, and the unprofitable nature of their classifications, drove Cullen into the admission that no sufficient distinction between pneumonia and pleurisy could be found, at least of a kind available at the bedside. The two diseases, therefore, which were described by the ancients, were placed by Cullen under the single genus, Pneumonia: and were only separated into distinct species in deference to prior authorities (*opinionibus et consuetudine medicorum aliquid concedere volens*).[†] In this respect Cullen only followed the example of Hoffmann; and though Pinel and his followers, tracing the first lines of an anatomical nosology, placed the two diseases widely apart, most of the great clinical observers of the eighteenth century[‡] justified Cullen's views on the subject, by showing that no close relation could be discovered between the symptoms and the anatomical distinctions. I shall hereafter point out the source of much of the confusion that prevailed in the eighteenth century, by showing that the modern idea, both of pneumonia and of pleurisy, is essentially different from the ancient, and leads necessarily to confusion when compared with it. In the meantime, it is sufficient to have shown, as an unquestionable fact, that up to the present century the study of morbid anatomy did not render in any degree more precise the nosological distinctions of pneumonia derived from the Greeks.

The art of percussion, however, as introduced by Avenbrugger and Corvisart, began to find followers about the beginning of the present century. From that epoch the anatomical characters of chest affections assumed a new importance as the elements of diagnostic distinctions. The tendency of the age, moreover, was towards the localization of disease to the uttermost; and the increased attention given to morbid anatomy tended to keep up the nosological definitions which had identified peripneumonia with hepatization or condensation of the lung. The labours of Laënnec confirmed these distinctions, but showed that the phenomena revealed by mediate auscultation were capable of affording new diagnostic assistance, and of recording the progress of pulmonic condensation with even greater exactitude than percussion. From the moment that crepitating râle, dull percussion, bronchial respiration and voice, became the recognised exponents of pneumonic inflammation, the symptoms assumed a secondary position in reference to its clinical history; the anatomical change in the lung was *the disease*, the symptoms were the accidental epiphenomena.

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[*] See the interesting chapter in Cœlius Aurelianus, Acut. Morb. II. 28; entitled, 'Quis locus in peripneumonicis patitur.'

[†] Synopsis Nosolog. Method. sub voce Pneumonia.

[‡] Especially De Haen and Stoll, who habitually, and almost constantly, use the compound word pleuro-peripneumonia, introduced by Vincent Baron in the preceding century, and in later times much employed by Andral.

This great revolution in the nosological idea of pneumonia could not fail to produce a marked effect upon the records of medical practice in that disease. The ancient peripneumonia was a disease always acute, always febrile, characterized by great functional disturbance, and great risk of suffocation. So fatal, indeed. was this disease, that even the pain which sometimes accompanied it was altogether subordinate to the danger.* The modern pneumonia is an affection which, as almost all the best observers have assured us, may vary indefinitely in its symptomatic characters, and not less as to its prognosis; nay, which may in some not unfrequent instances be altogether latent, as far as symptoms are concerned. The idea of a latent pneumonia would have appeared to Aretæus a strange contradiction, because in the symptoms was involved the very existence of the disease. To Grisolle a latent pneumonia appears scarcely admissible, because, although the symptoms may be entirely wanting or masked, condensation of the lungs can rarely occur so as to be unappreciable by physical signs.† The pneumonia of Grisolle might be no pneumonia, nor even any disease of the lungs, to Aretæus. I shall afterwards show, in relation to bronchitis, that the pneumonia of Aretæus might be no pneumonia to Grisolle.

It is remarkable that these facts, plain enough even on a superficial consideration of the subject of pneumonia, should have been so often overlooked by those who have compared the ancient with the modern practice in this disease. Laennec himself did not fall into this error. He was fully aware that cases of pneumonia, according to his own diagnosis, and according to that of his predecessors, were not comparable quantities; and he accordingly rarely attempts to compare them, either as to treatment or as to anything else. When, in stating the results of his own practice, he foresees that such a comparison is inevitable, he expressly warns the reader of the difference in the character of the cases, and particularly remarks, "*first*, that auscultation allows us to recognise pneumonia *much earlier* than we can do it by the observation of symptoms; and, *secondly*, that according to all appearances, many cases of simple pleurisy, or of pleuropneumonia with predominance of pleurisy, are necessarily comprised under the name of peripneumonia," in the accounts of the practice of his immediate predecessors.‡ This passage is of very great importance in reference to the history of pneumonia. Not only is it most creditable to the candour of Laennec, by whom it is put forward as an explanation of his own apparent success in treatment, but it is, in reality, one of the best statements in any author of the *nature* of the difference, in certain cases, between pneumonia before physical diagnosis, and pneumonia since that period. It may, therefore, fitly form the starting-point of a few more detailed remarks upon this subject.

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* Celsus, l. 4, c. 7, "Plus periculi quam doloris habet." See also Aretæus, l. 2, c. 1, De causis acutorum morborum.

† Traité de la Pneumonie. Paris, 1841, p. 434.

‡ Auscultation Médiate. 2nd edit., vol. i. p. 504.

A modern physician, in all respects well-informed and skilful, will form his diagnosis, and determine his treatment, in a case of pneumonia, chiefly from some combination of the following phenomena—viz., fever, local pain, dyspepsia, characteristic expectoration, cough, crepitating râle, altered percussion-sound, bronchial respiration, bronchophony, with perhaps the aid of some other less constant alterations of function, or of the physical signs, in peculiar and doubtful cases. He knows, however, that several of these phenomena may be absent in any particular case, and that one or two of them may be expected to be absent in a large proportion of cases. He will, therefore, not readily be induced to resign any of his means of exploration, and least of all the general or rational symptoms, which he will always regard as giving the chief indications for treatment, even where the diagnosis cannot be securely rested upon them. Such would, I think, be the verdict of most sound and experienced physicians, as to the competing value of rational symptoms and physical signs. It was evidently by these principles that Laennec was uniformly guided, as every page of his great work proves. No one has asserted these principles with more clearness and strength of conviction than Dr. Stokes. "In the cases we are every day called to treat," says he, "the value of physical signs must be tested by the history and symptoms, and these in their turn must be corrected by the physical signs. Whoever neglects either source of information will fall into the most fatal errors."* Similar statements are to be found in all the most trustworthy authorities. At the same time, it is quite evident that the invention of physical diagnosis has a tendency to diminish the *apparent* value of symptoms, not by circumscribing the field of their application in diagnosis, but by extending the field of diagnosis taken as a whole. It is also unquestionably true, that an undue estimate of the relative importance of physical signs has caused, in some minds, a neglect of the diagnostic value of symptoms; which is the more to be regretted, as it is so far removed from the spirit of Laennec himself, and the best of his successors.

It is natural to look to France for the first fruits of the new system; and I have already indicated that Laennec himself was the first to point out that "pneumonia" had, to some extent, changed its nosological character and its relations to medical practice, under the accessions to the means of diagnosis. I am the more anxious to direct attention to this point, because M. Louis has extracted from the words of Laennec above noticed, a meaning which it appears to me they cannot justly be made to bear; and because the commentary of Louis on the text of Laennec places in an instructive point of view the characters of pneumonia, as the disease was regarded by these two celebrated and excellent physicians. In his "Recherches sur les Effets de la Saignée," Louis refers to the passage of Laennec above quoted, and remarks upon it as proving, "that in a certain number of cases Laennec trusted to auscultation *exclusively* to indicate to him the existence of pneumonia; that crepitation, independently of every other local symptom, appeared to him to be sufficient for a secure diagnosis of this affection: so that he must have admitted cases of pneumonia among individuals who offered only crepitation, without rusty and semi-transparent sputa; without any alteration of the respiratory murmur; without any degree of dulness of the chest on percussion, at any point."[*] Upon the above estimate of Laennec's method of diagnosis, M. Louis attributes to him a want of sufficient caution in admitting cases of pneumonia, and maintains that he must have frequently mistaken the crepitating râle of bronchitis of the smaller tubes, for that of pneumonia. Without entering at present into the question whether this mistake was probable, or whether it would, as M. Louis supposes, account for the comparatively high apparent success of Laennec's practice, I think it may be safely said that nothing in the passage quoted by Louis, nor in any other portion of Laennec's work, goes to bear out the idea that he habitually assumed the existence of clinically important pneumonia, from an observation of auscultatory pneumonia *only*.[†] His assertion is simply, that auscultation allows of a *more early* recognition of pneumonia; in other words, that a crepitating râle in the lung, added to symptoms as yet not precisely characteristic, will enable an observer to predicate the disease with certainty. This is the only explanation of Laennec's meaning which his words require; it is likewise the only meaning consistent with the rest of his published opinions, which always describe the symptoms, and particularly the fever, as going along with simple uncomplicated pneumonia *from the beginning*.[‡]

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* Diseases of the Chest, part i. p. 40.

The above observations seem sufficient to show that M. Louis has inadvertently misrepresented the principles of diagnosis advocated by Laennec. Nevertheless, it seems that in relation to the important question of curability, there was an important difference between the "pneumonia" of Laennec and that of M. Louis. Under a similar treatment (by tartar-emetic in large doses), the latter lost three patients out of twenty, who were in good health at the time of the attack, while Laennec treated sixty-two patients with only six deaths, all of which fatal cases were in very unfavourable, and some in desperate circumstances at the time of admission. It is this discrepancy which M. Louis seeks to explain. I think it may probably (so far as it was not accidental) be explained as follows:—The cases of Louis were, in two points of view, *select* cases. They were, or appear to have been, a favourable selection for treatment, in respect that the individuals were all in good health at the time of seizure, and that all the cases springing out of pulmonary catarrh were carefully excluded.[§] On the other hand, they were all, *without exception*, cases of a serious character, considered with respect to the pneumonia itself; inasmuch as M. Louis, not content with a few conditions in fixing the character of the disease, seems to have demanded, as absolutely indispensable points in his diagnosis, fever, pain, rusty expectoration, crepitant râle, bronchial respiration, and more or less dull percussion—in other words, the proof of a pneumonia not merely begun, but continued to a dangerous extent.* It seems highly probable that Laennec, on the other hand, not being, like M. Louis, engaged in a strict comparative numerical "analysis of facts of the same species," and not in fact proposing to himself any rigorous analysis at all, gave all the cases which had satisfied *his own mind* of the existence of pneumonia, even at the earliest stage. Among these may doubtless be reckoned some cases of very slight pneumonia, and some, as M. Louis suggests, of "catarrhal pneumonia;" on the other hand, Laennec undoubtedly included some which were not in good health at the time of seizure, as his list of detailed fatal cases shows. Both Laennec and Louis have probably excluded the "peripneumonie des agonisans," and the "pneumonia" occurring as a mere complication of other severe acute diseases, with such symptoms as to be of secondary importance. On the whole, the estimate of the disease by these two great physicians does not appear to have differed in any material point; nor is there, I think, any ground for supposing that either of them allowed his judgment to be swayed considerably by the study of physical signs, to the exclusion of symptoms. M. Louis's exactness of method and conscientiousness of character place his facts beyond all shadow of suspicion in this respect, when viewed by the light of his own words. Laennec, speaking confessedly with less severe exactness, may sometimes have too lightly admitted some pseudo-pneumonic cases, on grounds to which I shall presently refer. But his object was simply to show that, in using the treatment by tartar-emetic for a series of years, he had lost very few cases *absolutely* (not comparatively, as the elements for a comparison did not then exist), in a large field for observation; and his method of showing this was eminently conscientious; he gives the whole of his cases, according to his own unbiassed judgment, and likewise the whole of his deaths, even when these were the result of complications, or of pneumonia itself a complication of other diseases, or of pneumonia treated *in extremis*. Further, he expressly warns the reader that his results are not fairly to be compared with those of his predecessors, and that, if so compared, they give the *stethoscopic* pneumonia an apparent advantage, which is not an advantage in fact.

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[*] Louis, Recherches sur la Saignée, p 65.

[†] This remark applies only to the second edition; in the first, Laennec seems, as might be expected, less alive to the risk of error in practice from a purely physical diagnosis. See Auscult. Mediate, 1st edit., vol. i. p. 170, par. 205.

[‡] "La pneumonie, *dès son début*, est accompagnée d'une fièvre aigue; il est *très-rare qu'elle manque ou même qu'elle soit peu intense*, et cela n'arrive guère que dans les pneumonies partielles très-peu étendues."—*Auscult. Med.*, vol. i. p. 436, in the 2nd edition, which compare with the passage quoted above.

[§] In his first series, composed, after this exclusion, of 78 cases of pure simple pneumonia, the cases excluded were 45 in number. As regards this exclusion, Louis says, "J'ai cru devoir écarter ces faits de mon analyse, pour que tout fût comparable. Aucun autre fait n'en a été écarté; de manière que j'ai réellement fait une énumération complète, ou l'analyse de tous les faits de même espèce que j'ai recueillés."—*Recherches sur la Saignée*, p. 9 (note).

I have next to inquire, whether the neglect of rational symptoms and exclusive attention to physical signs which M. Louis attributes to Laennec, have, to any considerable extent, interfered with the conceptions of other physicians of eminence, or biassed the judgment of the medical public, with respect to pneumonia. That M. Louis was aware of the danger of a fallacy from this cause, may be presumed from his having allowed the suspicion of error to arise in his mind with respect to Laennec's cases; for, it is in no spirit of detraction, but of genuine scientific caution, that he states his convictions on this subject. Further, M. Louis was one of the earliest observers who described accurately in the adult, and in the course of fever, the peculiar affections of the lung which simulate pneumonia in that disease, without producing any of its characteristic symptoms; and, although he evidently considered these affections a peculiar kind of pneumonia, the sequel of slight pulmonary catarrh in enfeebled subjects, there can be little doubt that he learned from these cases the danger of admitting crepitation, and even dull percussion, to a level with rational symptoms in pointing diagnosis or indicating treatment.* From an examination of his own cases, it appears that his treatment was never modified by the observation of these phenomena; and in describing them he gives the following important warning:—"It is very important to know these results of auscultation, so as to avoid the grave errors into which I have seen a physician fall, who had little practice in auscultation, but was in other respects very skilful. Whenever he met with the râle of which I speak, in the course of a typhoid affection, even of slight intensity, he supposed, notwithstanding the trivial character of the dyspnœa, that he had to do with an intense pulmonary catarrh, and prescribed bleedings, *without success*."† These observations of M. Louis show very clearly the source of his criticisms on Laennec's supposed error, and demonstrate that they were by no means founded on an hypothesis invented to explain a difficulty, but on a fallacy actually observed in practice.

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* Recherches, &c.; compare pp. 15 and 34.

M. Andral was the first author of repute who, after the publication of Laennec's great work, laid so much stress on the physical signs as to maintain the existence of pneumonia, *demanding active treatment*, in the entire absence of its characteristic symptoms. We have seen that Laennec himself did not do so; and this is made still more plain by a consideration of his article on symptomatic and latent pneumonia, and by some of his criticisms on the cases and inferences of Andral, in the second edition of the 'Auscultation Médiate.'‡ These criticisms show a disposition on the part of Laennec to vindicate to the utmost the application of the stethoscope to the diagnosis of pneumonia; but at the same time demonstrate his perfect freedom from that love of paradoxical and startling assertion, which has unfortunately, in the writings of many advocates of physical diagnosis, formed the apology of the sceptic, and the bane of the art. He maintains strongly, perhaps too strongly, that pneumonia of clinical importance is always recognisable by physical signs, but guards with great care against the conclusion that pneumonia recognisable by physical signs is always of clinical importance. When speaking of one form of what he believed to be latent pneumonia, or pneumonia without symptoms (that which occurs in the dying), he speaks of the diagnosis as having only been made "pour exercer les élèves."§ I may here notice that M. Chomel's doctrine is more *conservative* (so to speak) than that of Laennec, or indeed that of any other modern French author, in regard to the necessity of symptoms, as supplementary to physical signs in the diagnosis of pneumonia. He says that the crepitating râle is sometimes wanting where evident pneumonia exists, and sometimes present over a certain extent of the chest in cases where the absence of symptoms compelled him to renounce the idea of pneumonia. "I will add," says he, "that viscid and sanguinolent sputa, without crepitating râle, are, according to my experience, a much more certain sign of inflammation of the lungs than crepitation unaccompanied by this kind of sputa;"—a statement with which I can most heartily concur, as well as with a practical caution which he gives elsewhere, regarding examination of the back of the chest in the pneumonia of debility, "where the least movement may bring on mortal faintness," and where "an honest physician will rather remain in an uncertainty which has no bad result, than compromise the last breath of life by a curiosity which, to say the least, is not well-timed."*

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* Louis, Fièvre Typhoide, Paris, 1841. Compare the chapter, 'Des Poumons,' in the first volume, with that of "De la Respiration," in the second, refer also to the cases quoted in the latter. The first edition of this work of Louis was published in 1829.

† Op. cit., 1st edit., vol. ii. p. 283-4. The above remarks refer more particularly to the dry sonorous râle, which was usually present out of proportion to the symptoms. But the coexistence of crepitation with this râle is mentioned immediately afterwards as occurring in a good many cases (*un assez grand nombre*), without symptoms: and in p. 286, the same thing is stated with respect to six of the cases that recovered, in one of which, a young girl, who was very much enfeebled, the crepitation was extended, and lasted for a month, along with dull percussion at the corresponding point. But in this case, as in the others, the expectoration had no characteristic appearance."

‡ Op. cit., sect. ii. ch. 6, art. 2 and 5, vol. i. pp. 425, 478, *et seq.*

§ Op. cit., vol. i. p. 430.

We have seen that before stethoscopic examination became general, the name of pneumonia was applied almost exclusively to a class of cases having well-marked symptoms, and fatal chiefly in proportion to the intensity of the external phenomena. We have likewise seen that the use of physical diagnosis, and particularly the importance attached to the crepitating râle by Laennec himself, brought into view for the first time a new series of cases, in which the symptoms were no longer diagnostic phenomena, and in the discovery of which the stethoscope alone was to be trusted. This form of pneumonia was called by Laennec latent and symptomatic.† "It is mostly," he says, "when pneumonia is complicated with another disease that it may easily escape the attention of the physician." Frequently such affections were "rather agonies than diseases;" *péripneumonie des agonisans*, as Laennec termed them. Others were the consequence of catarrh; a consequence rare indeed in proportion to the frequency of this latter disorder, but not unfrequent in the variety long known as *suffocative catarrh*. Phthisis, eruptive and continued fevers, intermittent fevers, were likewise looked upon by Laennec as frequent sources of pneumonia, which was truly secondary and symptomatic, being masked in most cases by the primary disease. Such affections were occasionally, in young and robust subjects, accompanied by a notable increase of fever; but in old and exhausted persons, in whom they commonly occurred, these "latent" pulmonary inflammations were, on the contrary, coincident with sudden prostration, loss of consciousness, typhoid symptoms, and the commencement of the agony. In the intermittent fevers, owing to the slight mortality of these affections, their pathological anatomy was little known to Laennec.‡

In discussing the subject of collapse of the lung in the papers formerly referred to, I have adduced a large amount of evidence, showing, in detail, that the name of pneumonia, under which all these affections have been confounded up to a comparatively recent period, is singularly inapplicable, even in morbid anatomy, to a large number of them. The condensation of the pulmonary texture, and obstruction or temporary obliteration of the air-cells, which is common to them all as a physical phenomenon, has now been satisfactorily proved to be,§ in most of these cases, a mere mechanical change, sometimes connected with a certain degree of catarrh or bronchitis, but even more commonly the result of deficient evacuation of the normal or habitual bronchial secretion in enfeebled individuals. This altered view of the pathology of these affections clearly explains the latency of the so-called pneumonia; its connexion with typhoid, adynamic, and catarrhal conditions, and its familiar presence on the death-bed; while at the same time it shows what a fearful amount of havoc must have followed the application of antiphlogistic methods to a disease emphatically, in most cases, one of debility, when physical diagnosis too exclusively pursued, and erroneous pathology unsuspectingly followed, combined to mislead the physician. It is not an agreeable task to dwell on the serious errors of honest and able practitioners; but having taken a certain degree of trouble to point out rocks and shoals which have often caused me much anxiety, I think I should scarcely be justified in leaving them without a few beacon-lights for the benefit of future navigators. I shall, therefore, give a few examples of some misconceptions, which appear to me to have been most destructive in their tendency, and which, from the great and deserved reputation of the writings in which they occur, have been most widely influential in diffusing errors of theory and practice. It is satisfactory to reflect that they must have been often neutralized by the practical tact and sagacity of individual practitioners, and by the cautious and considerate spirit which commonly prevails in the practice of medicine at the present day.

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* Dictionnaire de Médecine. Art. Pneumonie, vol. xvii. pp. 231, 232. Paris, 1827.

† Laennec, op. cit., vol. i. p. 478, *et seq.*

‡ Op. cit., vol. i. p. 481, *et seq.*

§ See the paper referred to in the beginning of this article, in this Review for April; also my separate monograph, 'On the pathological Anatomy of Bronchitis, &c.'; Edin. 1850; and an excellent sketch of one large department of the subject in Dr. West's 'Lectures on the Diseases of Infancy and Childhood,' lect. xiii. and xiv.; from which sources the entire history of this complicated subject, so far as known to me, may be gathered.

I have already indicated M. Andral as having fallen under the criticism of Laennec, on account of some of his statements on the subject of pneumonia. Indeed, it is not improbable that Laennec himself had been led into similar errors, but had spontaneously rectified them, with that instinctive self-control which is conspicuous throughout his writings, and which constitutes one of the chief elements of his greatness. A comparison of the first and second editions of his work, as already indicated, would show that a certain number of statements, scarcely containing positive errors of observation, but liable to incorrect interpretation, were severely repressed in the latter. The observations, however, of M. Andral, show that while on the one side he perhaps fell into the error attributed to him by Laennec, of detracting from the value of physical signs as a means of diagnosis in certain forms of pleuro-pneumonia, he was far more chargeable with the opposite error of relying upon them, in cases where they appeared to present distinct indications, to an extent which Laennec would not have done. This will appear from the perusal of the chapter on "pleuro-pneumonia, with absence of one or several of its characteristic symptoms."* The forty-third observation is one of so called catarrhal pneumonia. From the absence of characteristic expectoration, the perfect integrity of the pleura, and the state of the bronchi as found in dissection, there is the strongest reason to refer this case to the category of simple bronchitis, with pulmonary lobular and diffused collapse. It was, however, distinctly an inflammatory affection, and was, therefore, treated correctly in principle. It is to be observed that the blood drawn was buffed, and the patient died asphyxiated, thereby attesting the real violence of the disease. In the forty-fifth case, which was not fatal, there was catarrhal expectoration, cough of three weeks' standing, slight dyspnœa and fever (no pain mentioned), the ordinary symptoms of the mildest bronchitis. The feeble habit of this patient, and the slight character of the symptoms, appeared to the physician to contra-indicate active treatment, while *a crepitating râle in the left side, with suppression of the respiratory murmur, and slightly impaired percussion*, seemed, on the contrary, to demand it. Observe the compromise of opposing principles! *Fifteen leeches* were applied to the *left side;* and next day it was covered by a *large blister*, which was caused to *suppurate.* A little broth was the only nourishment allowed."* There can be little doubt that this was a case of the simplest and slightest form of bronchitic collapse in an enfeebled patient, and requiring expectorants, or, at most, an emetic or two, for its cure. But M. Andral claims great credit for the stethoscope, which revealed in this instance a pneumonia that would otherwise have been certainly overlooked (*infailliblement méconnue*). "Such a mistake would have no serious consequence," he says, "if it did not frequently cause the employment of *proper therapeutic means* to be neglected." It will scarcely be contended by any one that the additional refinements in physical diagnosis, or the greater diffusion of its principles since the time of this case, have precluded the possibility or even probability of a similar employment of therapeutic means in the present day. But in the sixty-first observation, we see the principle of an exclusively physical diagnosis carried to its height. In a case of chronic diarrhœa, with debility and emaciation, treated by astringents and tonics with good effect for a month, the patient was carried off by a renewed febrile attack, followed, in a few hours, by a cadaveric expression of countenance. The complete absence of local symptoms left the cause of the relapse obscure. There was no cough or dyspnœa; but slightly impaired percussion and crepitant râle on the right side again subjected the patient to "proper therapeutic means"—viz., twenty leeches on the right side of the thorax, two blisters on the thighs, and, two days after, when the patient, having become progressively worse, was moribund, a blister to the right side. From the short account of the dissection, it seems impossible to say whether there was any inflammatory element in this case at all; but in the opinion of M. Andral, there is "no doubt that the pneumonia was the cause of the relapse and of the death."† It is scarcely possible to share in M. Andral's security of feeling in this point, at least to such an extent as to approve of the treatment.

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* Clinique Médicale, 3rd edit., vol. iii. p. 366, *et seq.*

The great and brilliant reputation of M. Piorry, as a follower in the footsteps of Laennec, invites an examination of his opinions on this subject, more especially as the profession owes to him a most acute and searching investigation into the nature of the *hypostatic* condensations of the lung, which include, for the most part, the cases of Laennec's "peripneumonie des agonisans." In his first researches into this affection, especially as it occurred in the Salpêtrière, M. Piorry was struck with its extreme frequency, both as a cause of death, and as a curable affection, accompanying almost all the diseases of old men, and disappearing during the convalescence. These researches satisfied him that this condensation or "pneumonia" was a peculiar disease, not developed (as had been supposed by some) after death, but distinctly to be traced, as indeed

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* Loc. cit., p. 405.

† Loc. cit., p. 457.

Laennec had shown, by auscultation and percussion practised on the living patient. Since the introduction of his novel nomenclature of disease, M. Piorry has resigned the name of pneumonia as applied to this condition, and now regards it as a congestion (engouement) of the depending portions of the lung, which has a great tendency, in some cases, to pass into inflammation, but is not necessarily, or *per sè*, inflammatory.* His remarks on the affection are, in many respects, striking and true; the latent character of the symptoms is admitted, as in Laennec's observations; the influence of catarrh on its production is also distinctly stated. The stethoscope is said to be an unfaithful guide as respects râles, but the absence or at least diminution of the respiration is said to be a valuable sign; and percussion, by indicating diminished clearness of tone, completes the diagnosis. But in the treatment, the phantom of congestion is scarcely less exacting a fiend than that of inflammation, and we hear, with horror, that although there is no indication for local bleeding, there being *no local irritation*, "the blood which congests the organ, and by its own proper weight accumulates in its lower portions, *must be removed;*" and accordingly "*general bleeding may be more or less frequently repeated, according to the effects which it produces.* . . . Moreover, when (by examination of the pulse, heart, &c.) we are assured that the circulation is languishing, this fact is *no reason for omitting to remove blood;* for the more considerable the column of liquid to be moved, the more motive power is required."† The organs of circulation are therefore to be stimulated by tonics and wine, while at the same time blood is detracted for the relief of this supposed congestion. The results of this truly heroic doctrine cannot be better exhibited than by an extract from the memoir of MM. Hourmann and Dechambre, upon the pneumonia of old men, which they observed, in common with M. Piorry, at the Salpêtrière.

"Venesection is indicated at the commencement of latent pneumonia, when a passive congestion has been the starting-point. The evacuation of blood in these cases has a treble effect: to remove a part of its material from this congestion, to combat the consecutive inflammation, and to diminish directly the asphyxial condition. *Unfortunately*, these indications, clear and rational as they are, cannot always be carried out;" the state of debility being the obstacle, on which ground the authors recommend the plan of stimulation and bleeding, mentioned above as that of M. Piorry, together with large sinapisms freely applied. The result must be given in the words of the authors:—"We have seen patients in whom the pulse was such as to require bleeding (le pouls invitait à la saignée) cease to expectorate *immediately after it had been practised, and die in less than twelve or fifteen hours.*"‡ A more graphic picture of a destructive practice was probably never given to the world; for the reader will observe that the retention of the expectoration is the direct *consequence* of debility, and the equally direct *pathological cause* of the "latent pneumonia," or collapse of the lung, which is here in question.§

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* See the chapter on *Pneumonemie hypostatique*; Piorry, Pathologie Idtrique, vol. iii. p. 401.

† Loc. cit. pp. 422, 423.

‡ Archives Générales de Médecine, 2^{e} série, tom. xii. p. 190.

§ To what extent this erroneous practice may have been followed out at the Salpêtrière, it is only possible to guess from the following data. MM. Hourmann and Dechambre repeatedly

The statistical returns of the Salpêtrière furnish, as has been shown in an able paper by M. Valleix, abundant proof of the frequency of this so-called "pneumonia" in the aged; and a comparison of the changes in these returns, over a series of years, taken in connexion with the pathological views known to be prevalent during the same periods, shows remarkably the revolutions of medical opinion in respect to pneumonia. Thus in 1819, when Laennec's work was first published, the adynamic and ataxic fevers of Pinel still held their ground, to some extent, in the nosology; gastro-enteritis had only begun to take its place among the fatal diseases of the aged, and pulmonary catarrh caused a considerable number of deaths; pneumonia, on the contrary, presented few deaths, averaging in this and the two succeeding years seventy-two per annum. With the rise of Broussais on the one hand, and the collateral increase in the knowledge of physical diagnosis on the other (from 1821 to 1829), pneumonia, encephalitis, and gastro-enteritis monopolized the field of nosology, and the ancient reign of idiopathic fevers was, to a great extent, overthrown. A still later period superseded gastro-enteritis by typhoid fever, and entirely supplanted the adynamic fevers of Pinel by pneumonia, which, owing to the assiduous study of crepitating râles and dull percussion, became emphatically *the* acute disease of the aged, and took to a considerable extent also the place of pulmonary catarrh. In point of fact, we find that pneumonia presented, from 1829 to 1839, more than three times the annual average of deaths in 1819-21; while the fevers so studiously classified and described by Pinel, in this very hospital of the Salpêtrière, as the overwhelmingly frequent disease of the aged, disappeared altogether from the returns.* We have little doubt that if the doctrine of collapse of the lung shall succeed in occupying the attention of our Parisian brethren, we shall see pulmonary catarrh restored to something more like its ancient position as a fatal disease among the aged, while pneumonia will be destined to another revolution; though whether the idiopathic fevers, so signally routed by the combined forces of Laennec and Broussais, will be permitted to collect their scattered forces under any new name, it would, perhaps, be useless to conjecture.

As to the diseases of children, I have already remarked that the differences of type in the "pneumonia" of early life have been the subject of much more careful and successful investigation than in the case of the similar affections of adults. Nothing can be more precise than the anatomical descriptions of the atelectasis, lobular pneumonia, carnification, état fœtal, &c., as it is described under different names by different authors.

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state that pneumonia was by far the most frequent fatal disease of the inmates of that hospital, and that its frequency among those affected primarily with other diseases, and recovering, was also very great; but that the immense majority (370 to 53) among those who recovered, were either latent affections, or at least presented no symptom of acute pneumonia, but only of "congestion." The manner in which these discoveries were made is also remarkable. The authors remark, "It is clear that physical signs only can prevent error, and we shall therefore announce a proposition, which will certainly not be contradicted by any physician who has practised for some time at the Salpêtrière. This is, that to prevent all danger of mistake, it is necessary to examine by percussion and auscultation the chest *of each patient at every visit*, whatever the affection be which has obliged him to enter the infirmary. There is scarcely a day in winter when there will not be found pulmonary congestions, and even true hepatizations" (condensations), "which could not have been otherwise suspected."—Loc. cit., p. 176.

* L'Union Médicale, vol. ii., Nos. 101-4-7; and Brit. and For. Med.-Chir. Review, April, 1849, p. 547.

The symptomatology of these affections was also studied with great care, even before their nature was rightly understood; and there has been much less misapprehension, in a practical point of view, of the latent and asthenic forms of pulmonary disease. M. Trousseau, one of the latest expositors of French pathology on this subject, has remarked that the true acute pneumonia, which scarcely occurs at the earliest ages, is beyond the second year so manageable a disease that he treated during six months twenty cases without a single death; whereas, the far more frequent catarrhal pneumonia was in his hands so fatal, that among thirty cases in hospital, not one survived.* I cannot help suspecting that M. Trousseau has here made a somewhat arbitrary division of his cases; and the pathological hypothesis by which he illustrates the two diseases, comparing the one to phlegmon, and the other to erysipelas, is by no means calculated to communicate clear ideas.†

In concluding this first and most complicated department of my subject, I shall endeavour to collect into a few words some general principles as to the pneumonic and *pseudo-pneumonic* condensations of the lung. It will easily be understood that these affections have been frequently confounded, although in well-marked cases, and in the dead subject, they are sufficiently well marked in their distinctions. In the *first* place, the connexion of the pseudo-pneumonia, or collapse of the lung, with catarrh, is such as, in some instances, to communicate to it acute symptoms resembling those of pneumonia proper; *secondly,* where it is not accompanied by special acute symptoms, it often occurs as a secondary phenomenon, in the course of acute diseases; *thirdly,* even where neither of these are present, the physical signs are such as to deceive the incautious observer, and to suggest the idea of a *latent pneumonia;* an idea which will cling to the mind of the practitioner, and will be suggestive of incorrect and deadly practice, in proportion as he has been accustomed to rely upon the refinements of physical diagnosis to the exclusion of symptomatic phenomena. In fact, not only have these affections been confounded during life by the most able and competent observers, but anatomy itself has been misled, and the scientific nomenclature of this science has been overrun with an immense number of useless and obscure terms, corresponding to the ideas of those who were vainly endeavouring to reconcile the simplicity of nature with the perplexing and complex principles floating in the contemporary literature of medicine, and involved in the general doctrine of some of its ablest teachers.

As regards the *pathological nature* of the pseudo-pneumonic condensations, it may be said that they all depend upon *collapse of the pulmonary air-cells,* in connexion with some degree of obstruction of the bronchi. This obstruction may be owing to an increase of the secretion from the mucous membrane; or, as is frequently the case in old and enfeebled persons, to an impaired excretion of the ordinary mucus, which accumulates chiefly in the branches near the root and central parts of the lung. In the latter case the resulting disease is totally devoid of any inflammatory or febrile character, and is found to be developed with an intensity proportionate to the debility of the patient. When, on the contrary, the bronchial secretion is increased and altered in character, a certain degree of febrile excitement may attend the disease, the amount of which is usually proportionate to the inflammatory character of the affection, and the amount of active treatment required. In some of the acute forms of pulmonary collapse, acute œdema, or even true pneumonic infiltration of the pulmonary tissue, may supervene, giving rise to a complicated type of disease, which is of course attended by many of the symptoms of pneumonia. Such cases are, according to my experience, very common in connexion with purulent infection after operations, and in the secondary inflammation after erysipelas or eruptive fevers.

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* Med. Gaz., vol. xlviii. p. 1089.

† It is remarkable enough, that in modern times pneumonia should have been regarded by many as *peculiarly* a disease of old age and infancy. Aretæus speaks of it as "for the most part fatal to youth and persons in the flower of life."—*De Morb. Acut. Causis,* lib. ii. 1. The examination of the disease in detail, as understood by the ancients and moderns, explains this discrepancy.

The *symptoms* of the pseudo-pneumonic condensations, as distinguished from those of true pneumonia, are often sufficiently instructive, and always of the greatest importance in guiding treatment. In many instances, it is true, the information derived from them is chiefly negative; nor is collapse of the lung, like pneumonia, to be regarded as a separate nosological condition, having a series of tolerably constant physiological phenomena. Its external manifestations vary, both in character and in amount, with the conditions that give rise to it, and are, as we have seen, often absent; the pulmonary collapse being, in these cases, only the secondary sign of a general constitutional condition. In other cases it has the symptoms of acute bronchitis carried to the highest degree of intensity; and in these instances it is to be observed that the *dyspnœa* is a far more striking symptom than in the true pneumonia, especially when considered in relation to the amount of condensation discoverable by percussion and auscultation. The reason of this disproportionate dyspnœa I shall state in the sequel; the fact is well known to all experienced physicians, as an important one in the diagnosis and prognosis of pneumonia and bronchitis.

Two other facts in connexion with the symptoms deserve notice. *Pain* is rarely, to any considerable extent, a symptom of the pseudo-pneumonic condensations—if we except the dull oppressive pain in coughing, which is so well-known as the character of bronchitis. On the other hand, I believe that if we separate from the pneumonic cases those which have been erroneously so described, very few instances will remain of *acute* pneumonia unattended by tolerably acute and distinctly localized pain at the commencement. This fact, no doubt, depends on the frequency with which the pleura is involved in the disease of the lung. It is, however, to be remembered, that acute pleurisy, with all its concomitants, sometimes occurs in bronchitis entirely free from pneumonic complication. Of this I have seen numerous instances, so that the rule as to pain must not be taken too absolutely.

The most important symptomatic peculiarity of pneumonia, as contradistinguished from the pseudo-pneumonic condensations, is undoubtedly the *expectoration*. That of pneumonia, in its ordinary acute form, is so well known that I do not feel myself called on here to describe it. That of the pseudo-pneumonic condensations is essentially *catàrrhal*, if indeed it be present at all. Often, as before mentioned, expectoration is absent; and I think I have even observed that expectoration which has existed habitually for some time before the attack may be suppressed, or materially diminished in quantity, on the supervention of the symptoms. It is certain that expectoration is frequently suppressed in the course of this affection; and MM. Hourmann and Dechambre have observed as a "singular fact" in "pneumonia" of the aged, that the cough and dyspnœa with which their patients had been habitually affected, occasionally disappeared during the prevalence of the signs indicating this disease.* The disappearance of cough and dyspnœa, as well as expectoration, only occurs, according to my observation, in the extremely asthenic forms of the affection, or in its latest stages.

The *presence of blood*, in one form or other, *in the expectoration of pneumonia*, has long and justly been regarded as a very characteristic symptom. I believe it to be so, for by far the greater number of cases of pneumonia in which the characteristic expectoration has not been observed, undoubtedly fall under the suspicion of belonging to the category of pseudo-pneumonia. But although the most characteristic types of pneumonic expectoration, tenacious, semi-transparent, and rusty, are little liable to mislead, the mere presence of blood in the sputum of catarrh does not necessarily indicate that hepatization of the lung is present. I shall return to this subject in the sequel.

The physical signs of the pneumonic and pseudo-pneumonic condensations bear in many respects so close a resemblance as to afford no sure and constant ground for a distinction. Crepitating râle is less frequently, or rather less constantly, present in the pseudo-pneumonic affections than in the commencement of true pneumonia; it is, moreover, in the former, frequently merged in the coarser and more audible catarrhal râles. Dulness on percussion is common to both affections in like measure; except that in the sthenic forms of pulmonary collapse it is always slight in proportion to the amount of the disease. In this case emphysema occupies the entire surface of the lung, at least at its anterior and lateral aspects, and leaves only the centre and root occupied by condensation. When condensation is discovered at the root of the lung, therefore, the symptoms being those of a febrile disease of respiration, the presence or absence of signs of emphysema is a valuable criterion, particularly if the disease has attacked a person formerly in good health. The value of this diagnostic peculiarity of the pseudo-pneumonic condensations is the greater, because, as I have shown elsewhere,† and as I am well assured by the most repeated and careful investigation, pneumonia, apart from other affections of the lung, has no tendency whatever to determine emphysema. The phenomena of the voice and of the respiration are not characteristic, as applied to the diagnosis of the pseudo-pneumonic condensations; I shall return to them, however, in treating of pleurisy.

On considering the whole of this subject, it will be seen that the diagnosis of the affections of which I have been treating is sometimes easy, on other occasions of no ordinary difficulty; and that it depends less on refinements of physical diagnosis than on an accurate knowledge of the whole phenomena of these diseases, and particularly of the varieties under differences of age, constitution, and other modifying circumstances. The indications of prognosis and treatment are to be drawn chiefly from the symptoms, the mode of access, and the progress of the affection; in general it may be said that the latent forms are never properly made the subject of local treatment, and that the general means employed should be always addressed to the symptomatic phenomena rather than to the physical condition of the lung as revealed by auscultation and percussion. It is also important to observe that the course of these affections in relation to treatment is singularly contrasted: in true pneumonia, the extension of the physical signs is the index of extension of the disease, and increased demand for active treatment; in the pseudo-pneumonic condensations, extended dull percussion is the index of deficient inspiratory power and general debility of the system; requiring tonics, stimulants, nourishment, or otherwise baffling art altogether. But on this subject also I have more to say hereafter.

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* Archives Générales, 2e série, vol. xii. p. 36.

† Pathological Anatomy of Bronchitis, p. 55.

(*To be continued.*)

Art. III.

Scarlatinal Dropsy. By John W. Tripe, M.D.

Few subjects in the whole range of medical literature have undergone a more complete and radical revision than that of renal dropsy; nor can we select any other disease to show more forcibly that the art of medicine, as well as science generally, has of late years made great and constant progress, and has been marked by great corresponding change. But whilst every advance in the sciences, and especially in the exact, forms a substantial basis on which to erect the future building, it is often the reverse in our comparatively uncertain art, in which crude theories have often been propounded for, and accepted as, well-established facts. But this is now less likely to happen, as we invoke, in most investigations, the aid of the microscope, chemistry, physiology, pathology, and of a more rigorous system of statistical inquiry. The great and certain assistance afforded by these is shown in the history of renal dropsy. Thus, until Dr. Bright brought the aid of chemistry to the examination of the urine, the connexion existing between certain forms of dropsy with albuminous urine and renal disease was not known; and until later observers examined the kidneys with the microscope, the continued presence of albumen in the urine was supposed to arise from one disease only, which was named from the discoverer, Bright's disease. And even now, the number, forms, peculiarities, and symptoms of renal diseases are, and will probably remain, unsettled for years. To prevent misconception, it may be as well to state, that this paper was written chiefly with the view of bringing before the profession the results obtained by a very extended statistical inquiry regarding the influence exercised by various circumstances on the mortality of scarlatinal dropsy. It will also embody, within a short compass, an account of the pathology, varieties, symptoms, complications, and treatment of the disease.

The present article will be devoted to a consideration of scarlatinal dropsy in its most extended sense, including all forms of albuminous effusion, whether it take place into one or more of the serous cavities alone, or involve the cellular tissue also: I, therefore, employ the term "dropsy" to signify the effusion of a non-inflammatory albuminous or fibrino-albuminous fluid into the areolar tissue, the serous cavities, or into both.

When the effusion primarily involves the areolar tissue, it usually first affects the eyelids and face; sometimes the back of the hands, or dorsum of the feet and ankles; sometimes the scrotum and penis; but wherever it may commence, it ordinarily extends, after a longer or shorter period, to the whole surface of the body, and often to one or more of the serous cavities. When the serous cavities are primarily attacked, that of the abdomen is most frequently selected; that of the meninges of the brain next; then of the pleuræ; and lastly of the pericardium. Frerichs* states, that in Bright's disease anasarca is the most frequent form of dropsy; next ascites; and then hydrothorax of both sides of the chest; that of the left being usually largest. He also states that the œdema of the lungs is a common complication, and is frequently accompanied by hydrothorax. Effusion into the pericardium was observed in 14 out of the 169 cases observed by Bright and Malmsten. Out of 680 fatal cases of scarlatinal dropsy which occurred in London during the year 1848, 592 were returned as anasarca; 38 as hydrothorax; and 50 as hydrocephalus. The proportion of males and females was as follows, during the last half of the year:—

TABLE I.†

	Males.		Females.		Total.
Anasarca	290	...	188	...	478
Hydrocephalus	26	...	24	...	50
Hydrothorax	18	...	20	...	38

Of the cases classed together as hydrothorax, two were of hydropericardium only.

The dropsical fluid varies very considerably, being much richer in albumen in that of the pleural cavities than of any other. The specific gravity varies from 1·005 to 1·014; the saline ingredients from 5·2 to 10·8; and the albumen from 3·60 to 38·0 in 1000 parts respectively. The following table shows this more satisfactorily.

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* Die Bright'sche Nierenkrankheit und deren Behandlung; and see British and Foreign Medico-Chirurgical Review, vol. ix.

† The materials for this table, and for nearly all the others, were obtained by a personal examination of the returns of "Causes of death" supplied to the registrar-general by the district-registrars, and of other unpublished documents at Somerset House. The deaths within the bills of mortality for London during the year 1848 were 57,628 from all causes, and 1756 from scarlatina. Each return comprises for this and every subsequent year a statement of the age, sex, occupation and residence, of each person, and also an enumeration of the disease, its secondary affection, and the duration of each. The age, sex, and residence must be correct; and in the case of scarlatina the duration also, as the rash is too remarkable a symptom to pass unnoticed. The duration and period of onset of some of the complications are open to a much greater chance of error, as they might escape observation for some days, but this objection cannot apply to anasarca, as it is too patent to pass unobserved. I have examined upwards of 80,000 of these returns, and have compiled all the tables, some of which, as those including the five years 1848—52, are formed from the returns of 284,306 deaths from all causes. Of these 284,306 deaths, 1600 were caused by scarlatinal dropsy (see Table 8), or in the proportion of 5·6 deaths from it to 1000 from all causes. I have much pleasure in publicly rendering my thanks to the registrar-general, and to Dr. Farr, for the readiness with which they afforded me every facility for making these investigations. I have subjoined the form (No. 1) of the medical

TABLE II.—*In* 1000 *parts.*

PLEURA.			PERITONEUM.			CRANIAL CAVITY.			SUBCUTANEOUS CELLULAR TISSUE.		
Albumen.	Salts.	Author.	Albumen.	Salts.	Author.	Albumen.	Salts.	Author.	Albumen.	Salts.	Author.
28 50	7 55	Schmidt	23·8	10 8	Marchand	10·03	9·69	Schmidt	Very small	10·1	Heller
26·12	7·64	Ditto	11·3[illegible]	9·77	Schmidt	7·98	8·46	Ditto	Ditto.	8·84	Ditto
25·64	8 30	Frerichs	3·9	9·03	Ditto				5·4	15·62	Ditto
24·9	7·80	Ditto	8 12	8·08	Kæfer				7·0	9·1	Simon
			38·0	[illegible]·06	Percy				3·60	7 70	Schmidt

It is more than probable that the great variation in the composition of the fluid depends in part on a greater impediment existing to the circulation in one case than in another; or to the presence of inflammation; as either of these will cause an increase in the albumen. The fluid, also, often contains urea; and Frerichs states usually in greater quantity than the blood; he also remarks, that the urea is converted, under certain circumstances, into carbonate of ammonia. But the various experimenters are by no means agreed either as to the quantity of urea when present, or to the frequency with which it occurs. Thus, Heller found it absent in

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certificate, and that (No. 2) of the return from each district-registrar to the registrar-general. The names are fictitious.

No. 1.

TO THE REGISTRAR OF THE SUB-DISTRICT IN WHICH THE UNDERMENTIONED DEATH TOOK PLACE.

Certificate of Cause of Death.

I hereby certify that I attended John Jones, aged 8 years last birthday; that I last saw him on January 11th, 1847; that he died on January 12th, 1847, at 7, King-street, Marylebone; and that the cause of his death was

	Cause of Death.	*Duration of Diseases.*
(*a*) First	Scarlatina .	42 days.
(*b*) Second	Anasarca .	28 days.
	Erysipelas .	2 days

Signed . . . **EDWARD LAWRENCE,**

Prof. Title . **M.D.**

Address . . **15, Soho-square.**

No. 2.

When Died.	Name and Surname.	Sex.	Age.	Rank or Profession.	Cause of Death.	Signature, Description, and Residence of Informant.	When Registered.	Signature of Registrar.
Jan. 12	John Jones.	Male	8 years.	Father a carpenter.	Scarlatina, 42 days Anasarca, 28 days Erysipelas, 2 days (Certified.)	Martha Hill, Nurse, 14, King-street, Marylebone.	Jan. 15	W. Howard

three cases, and present in one. As regards quantity, Marchand found 4·2; Frerichs, 1·62, 1·05, and 1·45; Simon, 1·2; and Rees, 0·415, and 0·15—respectively, in 1000 parts of the effused fluid.

The *causes* of the disease may be divided into the *predisposing* and *exciting*.

As it would occupy a very considerable space to discuss the influence of the scarlatinal poison, as a *predisposing cause*, we must pass it over comparatively unnoticed. The poison is considered by some authors as the essential cause of the dropsy, and by others as exercising but little direct influence on its production. The former consider that it acts on the kidney in the same way as on the skin; whilst the latter deny any such action. It is, however, quite certain that dropsy follows an attack of scarlet-fever more frequently than of any other febrile disease.

Certain atmospheric peculiarities; the epidemic or stationary constitution of disease generally, by which certain organs are predisposed to be attacked rather than others, may be enumerated amongst the most powerful predisposing causes. An examination of the history of scarlatina shows, during some epidemics, that a large proportion of the patients suffered from subsequent dropsy, whilst in others but few were attacked: that in one epidemic the dropsy was exceedingly severe, in others very slight; and these facts are clearly shown in Tables 14 and 15, by which it is proved that the per-centage of fatal cases of dropsy varied, in the years 1848-52 from 7·0 to 20·2, in each 100 deaths from scarlatina. The former per-centage is that for the first quarter of 1848, the latter for the last quarter of 1848.

Bad living, imperfect ventilation, damp and low situations, not only act as predisposing causes, but also materially increase the fatality of the disease, if the patient continue to be exposed to their influence.

Having thus briefly enumerated the predisposing causes, we next propose to consider, in a few words, the influence of the following *exciting causes*,—viz., cold, warmth, and stimuli, and of certain other modifying circumstances, on the production of, and the mortality from, scarlatinal dropsy. These modifying circumstances are (*a*), *variations in the atmosphere*, including changes in the temperature, humidity and electricity of the air; (*b*) *sex*, and (*c*) *age*.

Cold.—When cold is applied, after an attack of scarlatina, to the surface of the body, it acts very prejudicially, and is one of the most frequent excitants of the disease—or, at any rate, is more often blamed than any other. That cold and wet will have this effect is satisfactorily shown by albuminous dropsy frequently following exposure to their action without any previous febrile disease.* But that they act as supposed by Dr. Johnson, by checking "the process of desquamation and of elimination from the skin," is more than doubtful; for Schneemann and Mauthner assert dropsy to be most uncommon in those cases in which cuticular desquamation has been absent. It seems more probable that the dropsy is caused by the cold and wet impeding or suspending the proper functions of the skin, and by the kidneys (already congested by the scarlatina) being then called on to eliminate the materials ordinarily excreted by the skin.

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* I have had several cases of this kind under treatment during the present year (1853.)

That the skin and kidneys are vicarious in their actions, is proved by the experience of all ages. Thus, certain medicines, as nitre, will act on the skin if the surface be kept warm, but on the kidneys if it be kept cool. We should, therefore, expect that the greater the check to the cutaneous functions, the greater will be the extra duty thrown on the kidneys. It is also probable that the kidneys have greater difficulty in performing this vicarious action, than their own peculiar office; and that the impediment to the circulation in the glands is in proportion to the extent of such demand, and to the difference between the matters so excreted and the ordinary constituents of the urine.

Excess of Warmth.—But whilst the keeping a patient in bed, or at any rate in a room of an equable and moderately warm temperature, is the best prophylactic against an attack of post-scarlatinal dropsy, yet too great solicitude may, by an excess of temperature, entail defeat on our plans, and induce that which the treatment was intended to prevent. I have seen several instances in which confinement to a close, hot, ill-ventilated apartment was apparently one of the exciting causes of an attack.

Want of Ablution.—An absence of due ablution after an attack of scarlet fever often acts very prejudicially, and especially amongst the poor, as the parents usually fear some imaginary ill from washing children who are or who have been suffering from an exanthematous disease.

Stimuli.—Many are strongly opposed to the administration of stimulants during the eruptive stage of scarlatina, from a belief that they predispose to an attack of subsequent dropsy; but although I administer stimuli very freely in all the low forms of the disease—even in many instances whilst the rash is still out—yet in but few cases where due attention had been given to ablution with soap and water, proper ventilation, and freedom from cold, have I met with dropsy subsequently. Indeed, in no case of the malignant forms, in which stimuli were administered very early, have I had such a result. But that stimuli given during convalescence, or at any other period, when not required by the state of the system, may induce an attack of dropsy, I freely admit. It has been stated that alcoholic stimuli increase febrile action, and diminish all the secretions except those of the kidney, and that they, therefore, in scarlatina, divert the poison to the kidneys, and excite disease there. But it is by no means certain that wine or other stimuli invariably diminish the cutaneous functions in scarlatina; indeed, on the other hand, I have seen cases in which the skin has resumed its functions under their use. I allude particularly to those cases in which the skin is without unusual heat, and the pulse weak and small. Indigestible food may also be enumerated amongst the exciting causes.

Variations in the Atmosphere.—In addition to the agents just enumerated, variations in the atmosphere exert very considerable influence, if not on the supervention, yet most certainly on the mortality, of scarlatinal dropsy, as we shall see by an examination of the following tables, which show the mortality in the different seasons of the year. The first table shows, in each quarter of the year, the mortality (*a*) from scarlatina; and (*b*) from the consecutive dropsy; and (*c*) the per-centage of the mortality from dropsy, compared during corresponding periods with that from scarlatina:

TABLE III.

	1st Quarter, ending March 31.			2nd Quarter, ending June 30.		
	Deaths.			Deaths.		
	Scarlatina.	Dropsy.	Per-centage of dropsy to Scarlatina	Scarlatina.	Dropsy.	Per-centage of Dropsy to Scarlatina
1848	615	43	7·0	816	83	10·2
1849	776	118	15·5	497	67	13·5
1850	199	32	16·1	234	34	15·3
1851	206	28	13·1	169	20	11·8
1852	366	47	12·9	563	62	11·0
	2162	268	12·0	2279	266	12·3

	3rd Quarter, ending Sept. 30.			4th Quarter, ending Dec. 31.		
	Deaths.			Deaths.		
	Scarlatina.	Dropsy.	Per-centage of Dropsy to Scarlatina.	Scarlatina.	Dropsy.	Per-centage of dropsy to Scarlatina.
1848	1560	171	10·7	1765	317	17·9
1849.	386	58	15·0	446	90	20·2
1850	316	25	7·9	429	61	14·2
1851		(?)*	(?)	603	99	16·4
1852	668	68	10·9	952	152	15·9
	2930	322	10·9	4195	719	17·0

An examination of the table shows, in the March quarter, a variation in the ratio of the mortality from scarlatina (which was then at its minimum) and that from dropsy—the absolute mortality of the dropsy being 268, and the comparative 13 per cent.,† and the absolute mortality from scarlatina, 2162. It also shows that in the June quarter the mortality from scarlatina increased, whilst that from the dropsy diminished, both absolutely and relatively, being 266 instead of 268; and having a comparative mortality of 12·3 instead of 13·0 per cent. Also, that in the quarter ending September, although the deaths both from the scarlet fever and dropsy were much increased, yet the comparative mortality was less—the deaths from dropsy being 347,‡ and the comparative mortality only 10·9, per cent.; and that in the quarter ending December, the deaths from scarlatina were nearly double, and those from the dropsy nearly treble those of the first quarter - the comparative mortality from scarlatina being 36·3 to 28·7, and of the dropsy 17·0 to 13·0. And this difference in the mortality is rendered more obvious by ascertaining the per-centage of deaths, in each quarter, from dropsy. In the first quarter it was 16·8; in the second, 16·6; in the third, 21·7; and in the fourth, 44·9 per cent..

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* Some of the data were mislaid at Somerset House for this quarter.

† When the term "comparative mortality" is used in connexion with dropsy in any part of this paper, it is to be understood as meaning the rate or per-centage of deaths from dropsy to that from scarlatina. By 13 per cent., as above, is meant 13 deaths from dropsy out of 100 from scarlatina.

‡ One quarter was calculated—viz., that for 1851—as the data were mislaid at Somerset House.

respectively; the totals being 268 in the first quarter, 266 in the second, 347 in the third, and 719 in the fourth.

We may therefore say, that in proportion to scarlatina the dropsy is less fatal during the quarter ending September 30, and most fatal in that ending December 31; and that, absolutely and comparatively (when compared with itself), it is least fatal in the quarters ending March 31 and June 30, respectively, and most fatal in that ending December 31.

By pursuing this method still further, and examining the mortality in a similar manner for each month, we shall find that, both absolutely and comparatively, the deaths in August are much smaller than for any other month in the year. I regret much the comparatively limited extent of this table, as, not thinking it of much moment, I did not extract the monthly mortality for more than the two years 1848 and 1852; still, as these bear out the above statements, they will probably be deemed sufficient.

The first column of the table for 1848* proves that scarlatina prevailed chiefly during the latter half of the year, the mortality being under the average during the first six months of each year; the second column points out that the deaths from scarlatina dropsy were below the average in the first eight months, and above it in the last four. The fifth column, which shows the number of deaths from dropsy in comparison with those of scarlatina (the average proportion of the one to the other being about 1 to 7·6), is the most valuable for comparison, as it necessitates the inference that some causes must be in operation, either to increase the intensity of the scarlatinal virus; to direct it to the kidney; or to render the attack of dropsy more fatal at one period than another. In this column we see that in proportion to the mortality from scarlatina, the dropsy was below the mean in the months of January, February, April, June, and especially in August; and above it in March, May, July, and in the last four months of the year. And an examination of the corresponding columns for the year 1852† leads to somewhat similar results. The mortality from scarlatina and the dropsy was below the average in the first eight months, with the exception of June—when, as well as in the last four months, it was in excess; but when compared with scarlatina, the mortality from dropsy was in excess in January, June, and the last four months of the year. We here see, that although these two and the other years, as shown in the previous table, present one striking agreement—*viz., that the dropsy is most fatal, both absolutely and relatively, in the last four months of the year*, yet that they differ in this respect in several of the others. The question then arises, Will the meteorological elements of the table explain these peculiarities? A comparison of the first three months of one year with those of the other shows that they differed *in toto* as regards the comparative mortality of the dropsy (fifth column)—that of January, 1848, being 3·8 to 10·2 in January, 1852; of February, 1848, 2·4 to 7·7 in February, 1852; of March, 1848, 9·8 to 8·0 in March, 1852. The temperature for January, 1848, was much below the average, and the electricity unusually active; whilst the temperature for January, 1852, was high, and the manifestations of electricity unusually small. As regards the temperature alone, an examination of the 24 months shows it to have been above the average of the respective years

[Turn to p. 232.

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* See Table IV. on the opposite page. † See Table V. on the opposite page.

TABLE IV.—*Mortality 4 weeks in each month. Year 1848.*

	DEATHS.					METEOROLOGY.			
1848.	Scarlatina.	Dropsy.	Per-cent. of Scarlatina.	Per-cent. of Dropsy.	Per-cent. of Dropsy to Scarlatina.	Temperature of Air.	Rain in Inches.	Mean Humidity of Air.	Electricity.
January.	180	10	4·1	1·7	3·8	36·5°	1·2	·837	44
Feb.	192	7	4·4	1·2	2·4	43·2	2·6	·861	38
March.	186	26	4·2	4·5	9·8	42·5	3·1	·839	43
April.	205	20	4·7	3 5	6 9	49 9	3·4	·791	31
May.	227	31	5·2	5·4	9·5	55·9	0·4	·664	31
June.	348	29	7·9	5·0	5·9	58·7	3·5	·764	31
July.	378	47	8·6	8·1	8·7	61·5	2·1	·762	30
August.	480	28	10·9	4·9	4·0	58·5	4·0	·797	33
Sept.	602	85	13·8	14 7	9·9	55 8	2·4	·795	18
Oct.	653	122	14 9	21·1	13·1	52 1	3 5	·853	10
Nov.	499	90	11·4	15 6	12·6	43 2	1 2	·848	2
Dec.	434	83	9·9	14·3	13 4	42·8	2·5	·873	9
Total.	4386	578	100 0	100 0	100 0		30 5		320
Average.	365·6	48 1	8 3	8 3	8·3	50·2	2 5		26·6

TABLE V.—*Mortality 4 weeks in each month. Year 1852.*

	DEATHS.					METEOROLOGY.			
1852.	Scarlatina.	Dropsy.	Per-cent. of Scarlatina.	Per-cent. of Dropsy.	Per-cent. of Dropsy to Scarlatina	Temperature of Air.	Rain in Inches.	Mean Humidity of Air.	Electricity.
January.	119	18	5·1	5 9	10 2	42 0°	3·6	·847	3
Feb.	113	14	4·8	4·5	7·7	40 8	0·9	·879	21
March.	83	10	3 5	3·2	8·0	41·3	0 2	·810	35
April.	154	16	6·6	5·2	7·0	45·9	0 5	·757	8
May.	164	16	7·0	5·2	6 6	51·5	1 9	·773	24
June.	207	29	8 8	9·4	9·5	56·1	4 6	·767	28
July.	189	19	8·1	6 2	6·9	66·6	2·3	·732	34
August.	185	5	8 0	1·6	1·9	62·1	4·5	·728	34
Sept.	261	37	11 1	12·0	9·6	56·8	3·9	·772	10
Oct.	339	63	14 4	20·5	12·2	47·9	3·8	·803	0
Nov.	301	50	12·8	16·2	11·3	48·9	6·0	·828	0
Dec.	231	31	9·8	10·1	9·1	47·6	2·2	·794	0
Total.	2356	308	100·0	100 0	100 0		31·4		107
Average.	196·3	25·7	8·3	8 3	8·3	50·6	2·9		16·4

in 11 months, and below it in 13. During the 11 months in which the temperature was in excess, the comparative mortality was plus six times, and minus five times; whilst in the thirteen months in which it was minus, the comparative mortality was in excess six, and minus seven times. The months in which it was in excess had an aggregate comparative mortality of 85·6 and the months in which it was minus, of 114·4; showing that those months which were below the average temperature presented an excess of deaths from dropsy compared with those from scarlatina, in the proportion—after allowing for the different number of months—of 7·8 to 8·8. The months in which the temperature was minus the mean of 50·6° embraced, with one exception, all those in which the comparative mortality was in excess, as the following shows:

Table VI.

	Temperature Plus.			Temperature Minus.	
	Mortality.	Degrees.		Mortality.	Degrees.
	6·9	66·6		3·8	36·5
	1·9	62·1		7·7	40·8
	8·7	61·5		8·0	41·3
	9·5	59·9		10·2	42·0
	5·9	58·7		9·8	42·5
	4·0	58·5		13·4	42·8
	9·6	56·8		12·6	43·2
	9·5	56·1		2·4	43·2
	9·9	55.8		7·0	45·9
	13·1	52·1		9·1	47·6
	6·6	51·5		12·2	47·9
				11·3	48·9
Total	85·6			6·9	49·9
Average	7·8		Total	114·4	
			Average	8·8	

The above tables also indicate, that a temperature between 42·0° and 52·1° is the most adverse to patients suffering from scarlatinal dropsy, and consequently, that we should exercise the greatest care in our treatment when the mean temperature is within this range. To the above there was but one exception, in February, 1848, when, although the temperature was as low as 43·2°, yet the comparative mortality was only 2·4. But a comparison between the comparative mortality for the first quarter of 1848 and that of the corresponding quarters for the years 1849, '50, '51, and '52, shows it to have been much under the ordinary average: in 1848 it was 7·0; in 1849, 15·5; in 1850, 16·1; in 1851, 13·6; in 1852, 12·9. But although there is evidently a connexion between the temperature and the comparative mortality, yet an inspection of the table shows that there must be other causes in operation, as there is no accurate corresponding relation between the two; and the same has been observed of scarlatina. There is, however, a considerable difference in the range of temperature most fatal, which corresponds with the excess of mortality from scarlatina, and that which agrees with the excess from dropsy. As regards scarlatina my conclusions,* from an examination of the results of nine years, were as follow:

1. That a temperature below 44·6° is adverse to the progress of scarlatina; whilst a temperature above that point increases the mortality.

2. That the greatest absolute and comparative mortality for the year happened when the thermometer ranged between 49·6° and 56·9°; and the greatest decrease in the comparative mortality when the thermometer was below 40·0.

3. That the increase in the mortality did not occur in the same ratio with the increase in the temperature; nor did a diminution of the temperature take place in the same proportion as the decrease in the number of deaths; the closest correspondence happening in the months of December, January, and February, when the mean temperature was 40°0 or below.

These results may be presented at a glance thus:

49·6° to 56·9°	temperature corresponding with greatest mortality from scarlatina.
42·2° to 52·1°	,, ,, ,, from dropsy.

and it is as well to recapitulate that the last named temperature, 42·2° to 52·1°, corresponds not only with the largest comparative, but also with the largest absolute mortality from scarlatinal dropsy.

Electricity.—On comparing the dropsy with the manifestations of electricity, we obtain some remarkable results, for during the 10 months in which they were but seldom detected, the mortality was in excess in 9 months; and in the 14 months in which they were frequent, the mortality was minus in 10, and plus in 4 months. And on comparing the mortality of those months in which the electricity was active, with those in which it was passive, we shall at once see the remarkable difference:

	Comparative Mortality
Electricity active in 14 months . . .	98·6 = 7 1 each month.
,, passive in ten months . .	101·4 = 10·1 ,,

We may therefore conclude, that an active state of the electricity of the air exerts a favourable influence on scarlatinal dropsy. This agrees with my conclusion as to the influence of electricity on scarlatina—viz., "that an active condition of the electricity of the atmosphere is much more favourable to life, as regards scarlet fever, than a passive condition."

Rain.—The influence exercised on the body by moisture depends chiefly, as shown by Dr. Casper, on the co-existing temperature. I shall therefore divide the 24 months into the cold and wet, and the warm and wet—the cold and dry, and the warm and dry.

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* Medical Times; various numbers for 1849.

TABLE VII.—*Comparative Mortality. Years* 1848 & 1852.

	Wet.		
	Cold. — 50 5°		Warm + 50 5°
	2·4	....	5·9
	9·8	....	4·0
	6 9	....	13·1
	10·2	.. ..	9·5
	12·2	.. ...	1·9
	11 3	....	9·6
Total .	52 8	....	44·0
Average .	8·8	. .....	7·3

	Dry.		
	Cold — 50 5°		Warm. + 50·5°
	3·8	....	9·5
	12·6	....	8·7
	13·4	....	9·9
	7·7	....	6·6
	8 0	....	6·9
	7·0	....	—
	9·1	....	—
Total	61·6	... .	41·6
Average .	8·8	....	8·3

The temperature of 50·5° has been selected as being the average of the two years investigated. It will be seen that 12 months presented an excess of rain, being *plus* 2·5 inches per month for the year 1848; and *plus* 2·9 inches per month for the year 1852; and twelve months *minus* the same sums respectively. Of the twelve wet months, six were below the average temperature, and six above it; the cold and wet months were more fatal as regards the dropsy than the warm and wet, in the proportion (of the comparative mortality) of 8·8 per cent. to 7·3 per cent. Of the twelve dry months, seven had a temperature below 50·5°, and presented an average comparative mortality of 8·8, whilst the five warm months had an average comparative mortality of 8·3. The aggregate comparative mortality for the twelve dry months was 113·2, and for the twelve wet months, 96·8. We here see that the average mortality was less during the wet than the dry months; and least of all during the wet and warm months. This result agrees with that obtained when treating of the influence of humidity on scarlatina—viz., that, with the exception of March and December, the mortality was less in those months in which the degree of humidity was above the mean.*

Sex.—Having thus briefly considered the influence of appreciable atmospheric variations on the mortality of scarlatinal dropsy, we now proceed to another very interesting series of inquiries—viz., to ascertain the relative numbers in which the two sexes suffer; the mortality from the disease at different ages; and also briefly to consider the proportion of the dropsy to scarlatina. An examination of a large number of cases brings out the very important fact, that 60·3 per cent. of the fatal cases were of males, and 39·7 per cent. only were of females; 946 fatal cases out of 1575 being males, and only 629 females. Now that this great difference depends on the scarlatinal dropsy, ordinarily so called (I mean dropsy attended with serous infiltration of the cellular tissue), is shown by an analysis of 478 cases which were registered within the bills of mortality for London, during the last half of the year 1848. Of these 478 cases, 290 were males and 188 were females; being in the proportion of 60·6 males to 39·4 females. The number of males dying from scarlatina within the bills of mortality for London, during the year 1848, was 2473, and of females, 2294; or in the proportion of 51·8 per cent. of males, and 48·2 per cent. of females. The deaths from all causes in London, during the years 1838—44, under the age of fifteen years, were 85,028 males, and 76,600 females—making a total of 161,628; and a per-centage of 52·6 males to 47·4 females.* I will put these very remarkable results in a tabular form.

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* Medical Times, 1849.

TABLE VIII.

YEARS.		MORTALITY.			PER-CENTAGE.	
		Males	Females	Total.	Males	Females
1848-52†	Scarlatinal dropsy: all kinds	946	629	1575	60·3	39·7
1848	Scarlatinal anasarca	290	180	478	60·6	39·4
1848	Scarlatina	2473	2294	4767	51·8	48·2
1834-44	Deaths under 15 years, from all causes . .	85,028	76,600	161,628	52·6	47·4

We have here these most remarkable facts, that whilst the deaths under fifteen years, from all causes, present an average of 52·6 per cent. of males; and from Scarlatina, of only 51·8; those from scarlatinal dropsy amount to 60·3 per cent. It would be very interesting to solve this, but I fear our present state of knowledge is inadequate to the purpose—and especially as we find, by an analysis of 76 cases,‡ *that the proportion of males to females in those attacked is nearly the same as of those who die;* for, of the 76 attacked, 45 were males and 31 females; being 59·2 per cent. males, and 40·8 per cent. females. And to prove that this remarkable difference was not confined to any one year, or portion of the year, I subjoin the following table:

TABLE IX.—*Mortality. Scarlatinal Dropsy.*

1st Quarter:	Males.	Females.	3rd Quarter:	Males.	Females.
1848	18	25	1848	100	71
1849	73	45	1849	31	27
1850	19	13	1850	18	7
1851	16	12	1851	(?)	(?)
1852	38	9	1852	38	30
	164 = 61·2	104 = 38·8		187 = 58·1	135 = 41·9
2nd Quarter:			4th Quarter:		
1848	52	31	1848	197	120
1849	37	30	1849	53	37
1850	16	18	1850	42	19
1851	11	9	1851	66	33
1852	37	25	1852	84	68
	153 = 57·5	113 = 42·5		442 = 61·5	277 = 38·5

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* I have limited the age to 15 in this last inquiry, as nearly all the deaths from both scarlatina and scarlatinal dropsy occur under this age; wherefore a comparison with the per-centages obtained from a more extended range of age would lead to erroneous conclusions.

† The third quarter of 1851 is not included, for reason assigned p. 229.

‡ Private cases.

TABLE IX. (*Continued.*)

	Males.	Females.	Total.	Per-centage.		
				Males.	Females.	Total.
1st Quarter . .	164	104	268	61·2	38·8	100·0
2nd do. . . .	153	113	266	57·5	42·5	100·0
3rd do. . . .	187	135	322	58·1	41·9	100·0
4th do. . . .	442	277	619	61·5	38·5	100·0
	946	629	1575	238·3	161·7	400·0

The following presents the deaths in connexion with the attacks:

TABLE X.—*Scarlatinal Anasarca.*

	No. of Attacks.	No. of Deaths.
Males	45 = 59·2	290 = 60·6
Females	31 = 40·8	180 = 39·4
	76	470

Tables 9 and 10 show that the per-centage of deaths of males to females varies in the different quarters; that the largest proportion of females suffer in the second quarter, and the smallest in the fourth quarter—that of the second presenting the ratio of 42·5, and that of the fourth only 38·5 deaths out of 100 of both sexes; whilst the sum of males was 57·5 deaths in the second, and 61·5 deaths in the fourth quarter, out of 100 of both sexes. And an examination of the corresponding quarters for each year proves this variation to be more than accidental, as it occurred in nearly all. *We may, then, deduce from this examination, that males suffer from scarlatinal dropsy in the proportion of 60·3 per cent., and females of only 39·7 per cent.; that the per-centages vary between 61·5 and 57·5 per cent. for males, and 42·5 and 38·3 per cent. for females; and that these variations depend on some unknown but definite causes, which act with different degrees of intensity in each of the four quarters of the year.*

Age.—The age at which persons die from scarlatinal dropsy is early, as might have been expected from the parent disease being a malady of childhood. From an examination of the subjoined table, it will be seen that the fourth year (called *three* in the registrar-general's report) is the age at which it is most fatal.

TABLE XI.—*Mortality. Metropolis.*

AGE.	1848-51. SCARLATINAL DROPSY.		1848. SCARLATINAL ANASARCA.		1845-48. SCARLATINA.		1838-44. DEATHS—ALL CAUSES. UNDER 15 YEARS.	
	Total.	Per-centage.	Total.	Per-centage.	Total.	Per-centage.	Total Deaths.	Per-centage.
0	22	1·8	6	1·3	537	6·6	68,271	42·2
1	90	7·3	32	6·7	1170	14·5	32,940	20·3
2	144	11·6	66	13·8	1455	18·0	18,363	11·4
3	220	17·8	85	17·8	1373	17·0	12,042	7·5
4	180	14·6	71	14·0	1045	12·9	7,969	5·0
		= 53·1		= 54·5				
5	176	14·2	59	12·3		69·0		86·4
6	122	9·9	47	9·8				
7	99	8·0	44	9·2				
8	59	4·7	27	5·7				
9	41	3·3	17	3·6				
		= 40·1		= 40·6	1906	23.6	16,145	9·9
10	24	2·0	11	2·3				
11	19	1·5	4	0·8				
12	7	0·5	2	0·4				
13	8	0·6	1	0·2				
14	6	0·4	0	0·0				
		= 5·0		= 3·7	..	3·9	5,888	3·7
15 to 20	13	0·9	3	0·6	..	1·3		
20 to 30	5	0·4	3	0·6	..	1·0		
30 to 40	2	0·1	0	.. ..	..	0·8		
40 to 50	5	0·4	0	.. ..	..	0·4		
	1242	100·0	478	100·0	..	100·0	..	100·0

From the first and second columns we learn that 53·1 per cent. of the deaths from scarlatinal dropsy occur under the age of five years, and 40·1 per cent. between the ages of five and ten; making a total of 93·2 per cent. under ten years, and 5·0 per cent. between ten and fifteen, or a total of 98·2 per cent. under the age of fifteen, leaving only 1·8 per cent. above that age. We also see that 220 deaths out of 1242 happened in the fourth year, being 17·8 per cent. On comparing these with the next—scarlatinal anasarca, we find the proportion of deaths at an early age somewhat greater, being 54·5 per cent. under the age of five; 40·6 per cent. between five and ten, or 95·1 per cent. instead of 93·2 per cent. under ten; and 3·7 per cent. between the ages of ten and fifteen; making a total of 98·8 of all cases under this age. The fourth year is also the age at which the greatest mortality occurs, for 85 fatal cases happened at this age out of 478, or 17·8 per cent. The ratio of deaths from all forms of scarlatinal dropsy is the same; but the proportion of deaths, under one year, from anasarca, is smaller, being 1·3 to 1·8 per cent. from dropsy. It is also smaller in the second year, being 9·1 per cent. from dropsy, and 8·0 per cent. from anasarca. Anasarca, however, is most fatal in the third and fifth years, in the proportion of 28·7 per cent. to 26·2 per cent. In the sixth year, a marked difference is also perceptible, the dropsy being fatal in 14·2 per cent. to 12·3 per cent. from anasarca. These variations may perhaps in part depend on the greater extent of the tables of dropsy, which extend over a period of four years, whilst that of anasarca was formed from the results of half a year only. On comparing the deaths from scarlatinal dropsy with those from the parent disease, we find that only 1·8 per cent. happen from the former, in children under one year, to 6·1 per cent. from the latter; that 20·7 per cent. supervene from the dropsy in children under three years, and 39·1 per cent. from scarlatina; 53·1 per cent. from dropsy, and 69·0 per cent. from scarlet fever, in children under five years; and 40·1 per cent. from dropsy between the ages of five and ten, whilst only 23·6 per cent. are fatal from scarlatina during the same period. On comparing the mortality from scarlatinal dropsy with that from all causes under fifteen years, we find the deaths under one year to be only 1·8 per cent. of the former to 42·2 of the latter; under three years, 20·7 per cent. from dropsy to 73·9 from all causes; between three and five years, 32·2 per cent. to 11·5 per cent.; between five and ten years, 40·1 per cent. from dropsy to 9·9 from all causes; and between ten and fifteen years, 5·0 per cent. to 3·7 per cent.

Not having any data ready to ascertain whether or not the proportionate mortality from the dropsy is the same in the country as in London, I have arranged the following table of the mortality from scarlatina for all England, in opposition to that for London:

TABLE XII.—*Deaths from Scarlatina at Different Ages.*

——	Ages.											Total.
	0	1	2	3	4	5-10	10-15	15-20	20-30	30-40	40-50	
England ..	1291	3102	3705	3386	2677	5400	1056	268	228	130	56	21,304
Metropolis	537	1170	1455	1373	1045	1906	310	100	79	65	36	8,076
England ..	6·1	14·6	17·4	15·9	12·6	25·4	4·9	1·2	1·1	0·6	0·2	100·0
Metropolis	6·6	14·5	18·0	17·0	12·9	23·6	3·9	1·3	1·0	0·8	0·4	100·0

It will be observed that the variations are not very great, except at the age of five to ten, when it amounts to 1·8 per cent.; and we may suppose that the variations in the dropsy are not proportionably greater.

The last point which we have to consider respecting age is the influence which it exercises in the susceptibility to or mortality from the dropsy. It has long been believed, and, as the table will show, justly, that a proportionably less number of young children suffer from dropsy than of elder ones. The following table shows the number of deaths which occurred from dropsy in 100 cases registered as caused by scarlatina, at the different ages of life up to fifty years.

TABLE XIII.—*Deaths in the Metropolis.* 1848.

Age.	Scarlatina.*	Scarlatinal Dropsy.	From Dropsy out of each 100 Scarlatina.
0	302	10	3·0
1	645	40	7·3
2	848	69	8·1
3	812	125	15·4
4	652	92	14·1
5 to 10	1209	238	19·7
10 to 15	159	26	16·4
15 to 20	45	7	15·5
20 to 30	40	—	—
30 to 40	26	1	3·9
40 to 50	10	—	—

From this we see, that in the year 1848—which has been shown (Table III.) to have had a mortality from the dropsy considerably below the mean of the five years 1848 to 52—19·7 cases of every 100 registered as caused by scarlatina, in the quinquennial period five to ten years, were produced by dropsy; and that the smallest proportion occurred in children under one year, being only 3 per cent. The table also shows that the average mortality from the dropsy of all fatal cases of scarlatina under three years was 6·1; of the fourth year, 15·4; and of the fifth, 14·1 per 100, respectively; *or in the ratio of* 11·4 *in each* 100 *of scarlatina fatal under five years; of* 19·7 *in each* 100 *between five and ten; of* 16·4 *between ten and fifteen; and of* 15·5 *between fifteen and twenty.* We perceive from this that if a child suffering from scarlatina recover from the primary attack, he is more likely to die from dropsy between the age of five and ten than if he be under one year, in the proportion of 19·7 to 3·0; in the fourth year compared with the first in the ratio of 15·4 to 3·0; in the fifth, compared with the same year, of 14·1 to 3·0, or to the second, of 14·1 to 7·3; &c. The question arises, Does this immense difference depend on a less susceptibility to dropsy at an early age, or on death occurring during the eruptive period in greater proportion in young than in elder children? My own belief, from personal observation, is, that young children are less susceptible to the disease than older, but I have at present no data to solve the question.

From the preceding tables and considerations we arrive at the following conclusions: (a) *That the mortality under fifteen years of age, from all causes, is greatest during the first year of existence, being* 42·0 *per cent., and smallest between ten and fifteen years, when it is only* 3·7 *per cent.:* (b) *that the mortality from scarlatina is greatest during the third year of life, when it reaches as high as* 17·0 *per cent.;* (c) *that the deaths from scarlatinal dropsy are greatest during the fourth year of life, when they amount to* 17·8 *per cent.;* (d) *that although the dropsy produces the greatest number of deaths during the fourth year, yet compared with the mortality from all causes under fifteen years, it is by far the most fatal in the quinquennial*

*** Deaths from scarlatinal dropsy are included in the registrar-general's report under the head of scarlatina.**

period of five to ten years; (e) *that, from an average of four years, the period most fatal from the dropsy, when compared with that from scarlatina, is the quinquennial period of five to ten years, in the proportion of* 40·1 *per cent. of the former to* 23·6 *per cent. of the latter;* and (f) *that an accurate examination of the year* 1848 *gives the same result, in each* 100, *as* 19·7 *of all deaths by scarlatina during this period of life, were caused by dropsy.*

Proportion of Deaths from Scarlatinal Dropsy to Deaths from Scarlatina.—We have already shown that the proportion of deaths from dropsy to those from scarlatina differs in each month; and that although these variations may in some measure be accounted for by changes in the atmosphere, yet that some other cause or causes must be in operation for their production. It has been shown—with the exception of January and February, 1848, when the electricity of the air exhibited more numerous manifestations of its presence than during any other months of the two years 1848 and 1852 (save in March, '48)—that the month of August presented the smallest comparative mortality; and that this was the more remarkable from the months of July, September, and October presenting a large comparative mortality. It has also been shown that the comparative mortality was larger during the last four months of the year than in any other. That these variations in the comparative mortality do not depend altogether on the same causes as those which influence the mortality from scarlatina is apparent from an investigation of the five years 1848—52. On examining this period, we find that in 1848 and 1852, when scarlet fever was unusually fatal, the proportion from dropsy was below the mean of the five years; and that in 1852 it was smallest. This will be seen by a glance at the following table:

TABLE XIV.—*Comparative Mortality from Dropsy.*

1848	12·9
1849	15·3
1850	12·9
1851*	13·8
1852	12·1

The proportions for the quarters of the years 1848—52 were—

TABLE XV.—*Dropsy. Comparative Mortality.*

-	1st Quarter.	2nd Quarter.	3rd Quarter.	4th Quarter.
1848	7·0	10·2	10·7	17·7
1849	15·5	13·5	15·0	20·2
1850	16·1	15·3	7·9	14·2
1851	13·6	11·8	10·9	16·4
1852	12·9	11·0	10·2	15·9
Average.	13·0	12·3	10·9	17·0
Or,	24·6	23·3	19·8	32·3

One quarter calculated.

We here see that the comparative mortality was least during the first and second quarters of those years—1848 to 1852—in which scarlatina was epidemic; was in the same year below the average in the third quarter, and also in the last quarter of 1852; but above it by 0·7 in that of 1848. We also perceive that the comparative mortality was only 19·8 per cent. in the third quarter, whilst it was 24·6 and 23·3 in the first two quarters, when scarlatina was less prevalent than during the third. A comparison of the absolute mortality from the dropsy with that from scarlet fever also indicates the same. In the first quarter there were 268 fatal cases of dropsy out of 2162 from scarlet fever.; in the second, 266 out of 2279; in the third, 322 to 2930; and in the fourth, 719 in 4195. Or in the proportions of—*dropsy*, 17·0 per cent. in the first quarter; 17·0 per cent. in the second; 20·4 per cent. in the third; and 45·6 per cent. in the fourth: and of *scarlatina*—18·7 per cent. in the first quarter; 19·7 per cent. in the second; 25·3 per cent. in the third; and 36·3 per cent. in the fourth. We here see that the relative proportions between the two do not correspond during the year, but agree most closely in the first, and differ most widely in the last quarter; and that 1600 fatal cases of dropsy occurred in the five years 1848—52 in 11,566 fatal cases of scarlatina, being in the ratio of 13·8 cases of dropsy in each 100 of scarlatina. And Tables 11 and 13 show that the ratio varies very considerably with age, being highest, 19·7, to each 100 for each year of the quinquennial period of five to ten years; and lowest, 3·0 to each 100 cases fatal during the first year; the proportion increasing, but not in any given ratio, from the first year to one of the years between the ages of five and ten; most probably to the tenth year.

We next pass on to consider *The Causes of Death* in scarlatinal dropsy, which are many, and may be divided into two classes; in the first we may place those which induce death by what may be termed (1) pathofusical* terminations, and in the other by (2) accidental. In the first we may class death (*a*) by pressure of the fluid on some vital organ, as the brain, lungs, or heart; (*b*) by deterioration of the blood and system generally; and (*c*) by uræmia and its consequences; in the latter we place all those cases in which death is induced by inflammatory disease of the viscera or their coverings. The data which have hitherto afforded such trustworthy information, and have been of so great value, become in this part of our subject comparatively valueless, as so few cases are registered with more than the primary and secondary diseases: that is to say, with more than "scarlatina" and the subsequent "dropsy." Still, as there are some returns which contained a full detail of the complications, and as many were verified by post-mortem examinations, I have compiled the following results, but do not offer them as a standard by which we can ascertain the ratio in which the various causes induce a fatal termination. I may again mention that of 680 deaths registered as resulting from scarlatinal dropsy, 38 were stated to have been caused by cerebral effusion, and 50 by thoracic effusion *per se*, that is to say, uncomplicated with general dropsy. I do not think we should allow so great a proportion to cases of cerebral effusion, for but few were verified by post-mortem examination; but should rather place very many of them to the account of uræmia, as coma and convulsions are symptoms common to each. And it is more than probable that many of the deaths registered as caused by thoracic effusion, were really induced by inflammatory disease of the pleuræ or pericardium. It will, therefore, be impossible to present a table showing the ratio in which all the various causes of death prove fatal. The following, however, will afford some information on the subject, as it gives the cause of death in 128 cases.

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* From πάθος, *disease*, φῦσικὸς, *agreeable to nature*. I propose this term to signify those causes of death which result from disease in its ordinary course, as when death occurs from hepatization of the lungs in a case of pneumonia.

Brain and Spinal Cord.—Affections of these organs were the cause of death in 14 cases out of 128; meningitis being fatal in 6 cases; passive effusion into the cerebral cavity in 7 cases; and hemiplegia (cause unknown) in 1 case.

The Larynx was diseased in 3 cases, presenting 2 fatal from croup, and 1 from laryngitis.

Lungs and Pleuræ.—Diseases of these organs were fatal in no less than 40 cases of the 128, or in the ratio of 31·2 per cent. Of these, 3 were caused by bronchitis; 14 by pneumonia, 4 by pleuro-pneumonia, 7 by pleuritis, and 2 by pleuro-pneumonia with peritonitis, making an aggregate of 30 cases by inflammatory disease of the lungs and pleuræ. There were also 1 case of phthisis, 1 of œdema of the lungs, and 7 of non-inflammatory serous effusion into the thoracic cavity.

Heart and Pericardium.—Of the 128 cases, 12 were fatal from diseases of the heart and pericardium, 4 deaths being caused by pericarditis, 3 by endocarditis, and 5 by effusion into the pericardium without dropsy of other cavities.

Liver.—No disease stated; jaundice was a symptom in one fatal case.

The Stomach, Bowels, and Peritoneum.—The following is the analysis of 13 cases fatal from affection of these parts, out of the 128. Of these 13, 2 were cases of ulceration of the colon, with diarrhœa during life; 2 of ulceration of intestines (of which it was not stated), also with diarrhœa during life; 1 case fatal from diarrhœa, but registered without a post-mortem, and most probably fatal from ulceration; and 4 cases were fatal from peritonitis, making an aggregate of 9 cases from inflammatory disease. The other 4 cases were, 1 of disease of the mesenteric glands, and 3 of ascites without effusion into any other serous cavity.

Gangrene, Sloughing, Abscesses, and Erysipelas were fatal in 9 cases out of the 128, in the following proportions: 3 cases of abscess, 2 being of the neck, and 1 of the joints: 1 case of gangrene of the feet; 1 case of mortification (part not mentioned); 2 cases of suppuration of the glands of the neck; and 2 cases of erysipelas.

Death by Uræmia.—By this I mean, death by that class of symptoms so well known by the name of poisoning by urea. The prominent symptoms of uræmic poisoning were manifested in 37 cases out of the 128. Of these 37 cases, 27 were of convulsions, or of convulsions and coma together; 5 of coma; 1 of tetanic convulsions; 1 of epileptic convulsions; 2 of complete suppression of urine (convulsions and coma were enumerated as symptoms in these two cases); and 1 of uræmia.

Having now treated of, at what may perhaps be considered by many an unnecessary length, the influence of season, temperature, humidity, electricity, sex, and age, on the production and mortality of scarlatinal dropsy, and also the causes of death, we will next consider the alterations produced by the disease in the blood and urine, and also the symptoms and phenomena of uræmic poisoning. We shall then proceed to the last division of our subject—viz., a consideration of the other symptoms and peculiarities of scarlatinal anasarca; its diagnosis, prognosis, and treatment.

Alterations in the Blood.—When the scarlet-fever poison is taken into the system, it is generally believed that a peculiar action is set up in the blood, and certain changes effected, by which the phenomena of scarlatina are manifested. In some cases the poison speedily induces secondary diseases of various organs, in others not until some time has elapsed: under the former circumstances, the changes in the blood have been very imperfectly ascertained; in the latter they have been more accurately described. In some patients the alterations in the blood appear to cease when the febrile stage has passed away, but in others they continue; when it assumes a peculiar leuco-phlegmatic appearance, which is quite diagnostic of the approaching dropsy. Sometimes the dropsy sets in before the leuco-phlegmasia has made any progress; at other times not until it is well marked. The period at which it occurs will be considered in the next article.

The alterations in the physical characters of the blood drawn from the body are well marked, for we find the clot either rather small and buffed, or large, loose, and dark-coloured, swimming in a large quantity of greenish or opalescent serum. The appearance of the latter very much resembles that of blood drawn from a typhus patient. The opalescent or milky appearance of the serum, when present, may depend on the presence (*a*) of an albuminous substance in a state of minute division, as shown by Simon and Scherer; (*b*), of fat, also in a minutely divided state, as shown by Trail, Nasse, and others; (*c*) of colourless corpuscles suspended in the serum. Frerichs states one of the two former to be the cause of the turbidity in cases of renal dropsy, for he has met with both, and believes the ultimate cause to be the same in both cases—viz., a diminution of the alkalinity of the blood, by which, in the former, the albuminate of soda is decomposed, and the albumen set free; and in the latter by fat in combination with a less amount than usual of alkali.

The alterations in the serum are not very marked at first, but as the disease progresses, and in proportion to the quantity of albumen voided by the kidneys, its specific gravity diminishes, and the quantity of its saline constituents is reduced. These changes were well known to Christison, who stated the specific gravity to be gradually reduced from 1·030 to 1·022, and subsequently much lower. The quantity of solid matters in the serum he also states to be often reduced from 100 to 60 in 1000 parts: these statements apply, of course, to advanced cases of the disease. Frerichs analyzed the serum of a woman who had been ill with scarlatinous anasarca for eight days only, and found the specific gravity to be 1·019; the saline, fatty, and extractive matters about normal; but only 51·7 of albumen, instead of about 75·0 parts, which Simon considers the normal average. In the early part of the disease, Christison states that the quantity of hæmatosine is normal, being about 133 parts in 1000; and that the fibrine is either normal or more or less increased, sometimes considerably so. When the disease has lasted some time, the blood-corpuscles are much diminished in quantity, sometimes even to one third. Heller made a series of experiments on the blood of persons suffering from renal anasarca, and arrived at the conclusion that the albumen was the constituent chiefly diminished. Urea is usually present to a greater or smaller extent, but is sometimes altogether absent. Christison detected it on the ninth day of the disease. Heller detected it in two cases; in one case he found 1·85, and in the other 1·74 in 1000 parts of blood. Simon gives the analyses of thirteen cases, and states that a considerable quantity of urea was found in most. Garrod also detected uric acid in the serum in three cases of Bright's disease. MM. Becquerel and Rodier* have lately investigated very extensively the morbid changes in the blood in many diseases, and have drawn the following conclusions as regards "acute Bright's disease;" that the quantity of albumen is slightly and speedily reduced, and that the globules and fibrine remain unaltered. The changes in the blood may be summed up, as consisting, in the acute stage, of (*a*) a diminution in the quantity of albumen, (*b*) a slight increase in that of the fibrine, and (*c*) in the presence of urea. In the more advanced stage, by (*a*) a diminution in the quantity of blood-corpuscles, and (*b*) of the albumen, (*c*), a normal or increased quantity of fibrin, and (*d*) the presence of urea and uric acid.†

Alterations in the Urine.—As before stated, the urine usually contains, at some period or other of the eruptive or desquamative stage, a greater or less quantity of albumen for a shorter or longer duration. In some cases I have detected it in the urine in the morning and not in the evening, or *vice versâ; sometimes only in one specimen during the whole period of the disease.* Besides albumen, we also meet with a variable quantity of renal epithelium in the desquamative stage; sometimes fibrinous casts of the renal tubules, containing either epithelial cells or blood-corpuscles; and sometimes free blood-corpuscles. In some cases none of these abnormal constituents appear until the dropsy shows itself, but more generally they occur as above stated, and continue until the supervention of the dropsy. As a rule, when these abnormal constituents, or either of them, persist in the urine for more than ten days after the disappearance of the rash, and especially if they increase in quantity, we may expect an attack of dropsy.

In the first stage of dropsy the urine is at first high-coloured, bloody, or brownish red, scanty, of high specific gravity, often turbid from the presence of a large quantity of uric acid, of lithate of ammonia, soda, or potash, or all mixed, which it deposits on standing. On examining the precipitate we find, in addition to the lateritious deposit, if present, a variable quantity of abnormal cells—to wit, blood-globules, mucous corpuscles, renal and vesical epithelium more or less disintegrated, and large globular, nucleated cells, some of which resemble in size and constitution the parent cells of cancer. Another variety of large cells was often met with by me in 1848, but is rarely found now. They were of a yellow colour, with indistinct nuclei, and were usually met with in apposition with each other, sometimes forming a cluster, or a single row of four, five, or six; and more rarely, two or three rows joined together. When thus united they often separated from each other, and, as far as could be seen, without the rupture of any intervening membrane. They were of a more or less hexagonal shape, of less thickness than length, and of greater breadth than the fibrinous casts of Simon. In addition to these cells, the sediment usually contained a greater or less quantity of the fibrinous casts of the renal tubules (casts of Simon), which have lately been named by Johnson epithelial casts, granular casts, &c., according to their appearance and constituents. These casts are from $\frac{1}{700}$ to $\frac{1}{500}$ of an inch in breadth, and from $\frac{1}{50}$ to $\frac{1}{30}$ of an inch in length, and are composed of granular fibrine, either by itself or mixed with epithelial cells or blood-corpuscles. When containing blood-corpuscles, they are probably the result of rupture of the vessels of the Malpighian tuft, and consequent effusion of blood into the secreting tubules of the kidney; when containing epithelial cells, of the effusion of fibrine into the same tubes. Frerichs and Johnson are at total variance as to the mode in which the epithelial cells become an element of these casts. Johnson believes that the cells are thrown off from the parietes into the free canal of the tubules; whereas Frerichs states his belief that the cells are only accidentally mixed with the fibrine, being entangled with it. This will be considered more fully hereafter. These casts also sometimes contain crystals of uric acid or oxalate of lime, and sometimes are unmixed with cells or crystals, when they are very pale and almost transparent. The ordinary salts are sometimes decidedly deficient, at others about the normal standard. Frerichs states the former to be the most common; Rayer, the latter. Frerichs also says that the diminution in the quantity of the salts will depend on the greater or lesser number of the tubes blocked up. The urea is usually diminished in quantity; Frerichs states it to vary from 7·9 to 14·2 per 1000; Simon, 7·7. We may roughly estimate the quantity of urea present by adding nitric acid to urine on a slip of glass, and then observing, under the microscope, the quantity of nitrate of urea formed. We also sometimes meet with common salt in combination with urea, in the form of octohedral crystals, which closely resemble oxalate of lime. They may, however, be readily distinguished from the oxalate by their larger size and rounder angles, and by their dissolving in distilled water. When dissolved, they often recrystallize in cubes instead of octohedra; and if treated by nitric acid, dissolve with slight effervescence, producing crystals of nitrate of urea.

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* Gazette Médicale de Paris.

† See Simon, vol. i. p. 322; and vol. ii. p. 515; also British and Foreign Medico-Chirurgical Review, vol. ix. p. 301; and other authorities quoted.

The specific gravity varies very much. I have met with it, at the commencement of the disease, as low as 1·010, and as high as 1·048; in the latter case the urine became solid on the addition of nitric acid; but the average of my cases, whilst the urine was scanty, was from 1·020 to 1·025, and this may be considered as the mean of other observers. The albumen varies as greatly as the specific gravity, ranging from 7·86 to 33·61 in 1000 parts of urine. The last-mentioned amount was that given by Simon, and the average may be taken at about 16·5 per 1000. The weight of the albumen thus passed in the twenty-four hours ranges from 75 to 387 grains; the average may be taken at 170. The weight of the fibrinous cylinders varies very much, and is ascertained with difficulty from their intermixture with other deposits. Frericks found it 6·0 in 1000 parts.

As the disease advances, the urine alters considerably; it is passed less frequently and in greater quantities, is much lighter in colour, gradually assuming the peculiar greenish tint so indicative of the presence of albumen; when gently shaken, it often looks like thin syrup, and froths very much when more forcibly shaken. The blood-corpuscles gradually diminish, and finally disappear; the mucous corpuscles and other nucleated cells, and the fibrinous casts, also diminish in number; the latter, if the disease assume a chronic character, frequently cease to contain either epithelial cells or blood-corpuscles. The renal epithelial cells are also voided less perfect, being more disintegrated, and as the patient recovers gradually disappear from the urine as well as all the other abnormal constituents. But if, during the progress of convalescence, any cause—as improper food, cold, the use of stimulants, &c.—should induce a recurrence of the acute form, the urine will assume its corresponding characters. We may assume as a rule, that the larger the number of blood-corpuscles, fibrinous casts, and albumen, and the less the amount of the urine, the more intense is the renal disease, the longer will be its probable duration, the less perfect the recovery, and consequently the greater the danger.

Uræmia.—One of the most formidable complications of scarlatinal dropsy is uræmia. When well marked, the symptoms are convulsions or coma, or both, occasionally delirium, with the suppression of, or a great diminution in, the secretion of urine; and consequent retention of the urinary constituents. But although coma or convulsions constitutes one of the predominant symptoms, yet in making a *post-mortem* examination, we rarely detect any morbid alterations. Sometimes when the symptoms have come on gradually, we meet with indications of slight inflammatory disease; yet, in the generality of cases, the quantity of fluid in the ventricles and arachnoid cavity, is but little if at all above that of health. In my own cases, I did not detect any morbid products, and Frerichs makes a similar statement, having never met with more than a slightly increased amount of fluid, the largest quantity not exceeding one ounce.

There are but few cases more difficult to detect, and yet more important to diagnose correctly, than these; as the uræmic symptoms may occur suddenly, and present all the ordinary features of apoplexy. Thus we may be told, on our arrival at the patient's bedside, that whilst in the pursuit of his ordinary avocations he was suddenly seized with coma and convulsions. This happened to myself in one most marked case; but on inquiry, I ascertained that many of the ordinary signs of the first stage had been present for some days. These may be enumerated as follows:—sleepiness or heaviness, headache or confusion of thought, dizziness, impairment of vision, dilatation and sluggishness of the iris, lassitude, dislike of motion, and other symptoms of *malaise.* In addition, the patient often complains of diarrhœa or vomiting (the latter more frequently than the former), and sometimes of both; but neither of them is so common in scarlatina as in other varieties of renal dropsy. Frerichs says although others have detected urea in the matters ejected from the stomach, yet that he has not, and on the contrary, has frequently ascertained the presence of carbonate of ammonia. I have not had any opportunity of verifying this observation. If a specimen of urine can be obtained, our diagnosis would no longer be obscure, as we should find it small in quantity, deep coloured, albuminous, containing lateritious deposits, and one or more of the other abnormities just described. The pulse may be either slow and full, 60 to 65, or small, quick, and soft; and the skin is usually dry and harsh, and sometimes desquamating. This latter is a most important sign in the comatose stage, as we cannot obtain any history from the patient, and rarely a specimen of his urine. Another important sign, under these circumstances, is a urinous or ammoniacal odour of the breath. The presence of ammonia may be detected by holding a piece of moistened litmus to the mouth, when it will indicate the presence of an alkali, and by a rod dipped in hydrochloric acid, which will give out white fumes when exposed to the breath. Frerichs says that he has repeatedly shown its existence in the breath of men affected with uræmia, and of animals into whose veins urea had been injected.

If we are called to the patient before the invasion of coma and convulsions, and can effect a permanent increase in the secretion of urine, we can ordinarily avert any further mischief; but if we cannot, especially in the advanced stage, increase the urinary secretion, or cause the ejection of the poison, whatever it be, in any other way, the attack will speedily prove fatal. These observations apply with greater force to the comatose stage.

Many theories have been proposed by as many authors; one believing the symptoms to be produced by arachnitis; another, by cerebral or spinal effusion; others, from the presence of urea in the blood, or of some other compound which acts as a poisonous agent on the brain and spinal cord. The theory of poisoning by urea has been most carefully investigated, and has been accepted by some, and rejected by others, amongst the latter of whom we may enumerate Dr. Rees, Jones, and Frerichs. The former rejects it from the want of correspondence between the amount of urea in the blood and the intensity of the symptoms; and also from the total absence of uræmic symptoms in cases where a large quantity of urea was detected in the blood. In addition to these, the experiments of Vauquelin, Segalas, Bichat, and others, of injecting urea into the blood of dogs, without any other effect than an increased flow of urine, may be adduced in opposition to the urea theory. Indeed, so marked was the diuretic action, that these experiments have led to the successful use of urea as a diuretic. Dr. Rees considers the symptoms of uræmia to be caused by a diminished density of the blood; but experience and observation show his theory to be less tenable even than the former. The last and most plausible theory is that lately propounded by Frerichs,—viz., that the urea is changed in the blood into carbonate of ammonia by the agency of a ferment; that if this conversion be effected suddenly, the symptoms will resemble those of apoplexy; but if gradually, they will assume the form of typhus, with subsequent coma and convulsions. To prove this theory, he performed the following experiments. He removed the kidneys of some animals, and then injected urea into their veins: at first no peculiar symptoms were observed; but after the lapse of some hours, they became restless, and vomited, when ammonia was detected in the expired air; and after a short time, convulsions, alternating with stupor and stertor, or stupor and coma, supervened. After death, ammonia was detected in the blood and in the contents of the stomach. He then injected a solution of carbonate of ammonia into the veins of other animals, when convulsions and stupor quickly came on, but ceased after a few hours, the ammonia having passed off by vomiting, and in the expired air, and probably also by the skin. I will not attempt to discuss the merits or demerits of either theory, as this paper is intended to be as much matter of fact as possible.

The diagnosis of uræmia is often very difficult; but the state of the skin, urine, breath, and the previous history, will, with ordinary care, suffice to distinguish it from any other disease. The treatment of uræmia will be considered in the next and concluding article.

Art. IV.

The Influence of Liquor Potassæ on the Urine in Rheumatic Fever. By E. A. Parkes, M.D., Professor of Clinical Medicine in University College, London, and Physician to University College Hospital.

The present paper is a continuation of one published in this journal in January, 1856, in which the action of Liquor Potassæ (Pharm. Lond.) on the urine in health was discussed. It was then shown that this medicine, when taken into the circulation, without being neutralized in the stomach, speedily passed out by the urine, and chiefly, though not entirely, in combination with sulphuric acid, the formation of which it for the time increased. It appeared a fair inference from the facts then stated, that the Liquor Potassæ, in the healthy system, acted as a destructive agent on the sulphur-containing constituents—i. e., the albuminous compounds of the blood or tissue, and led, among other less-easily recognised changes, to the oxidation and elimination of their sulphur. It was shown also that the quantity of water poured out from the kidneys was increased, as well as the organic substances, included under the vague term "extractives." It was, of course, apparent that the study of the action of the liquor potassæ in the healthy system was by no means completed, and that many points remained unexplored. It seemed, however, that a firm basis for future investigations had been laid down, and it was determined to pursue the inquiry on the urine of disease in the same way, and to pay special attention to the inorganic constituents of this secretion.

In the diseased body liquor potassæ is placed under different chemical conditions from those presented to it by a healthy system, and, therefore, no *à priori* decision as to its action in disease can be possible. Though the truth of this remark will be made sufficiently evident in the course of this inquiry, it will at the same time be found, that its action in health may be taken as the general rule of its action in disease. I must observe that I can pretend to do no more than to give, what I hope will be found accurate and trustworthy experiments towards the elucidation of a subject so vast that the labours of one man must be totally unable to exhaust it.*

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* I have not alluded to the observations of Dr. Golding Bird or others who have contributed so much to our knowledge of the action of the diuretic salts of potash. My present object forbids this; but I hope my temporary silence will not be construed into indifference to these useful inquiries.

On the present occasion I propose to give the results of experiments in four well-marked cases of rheumatic fever treated in University College Hospital. In future papers the effect of liquor potassæ in inflammations, and in some of the acute specific diseases, will be given.

The four instances of rheumatic fever were typical cases, with great pain and with the thermometer rising to 103° or 104° Fahr. Three were in men (aged 22, 23, and 26), and one was in a woman (aged 20). Three were treated with liquor potassæ alone, one with this medicine and with colchicum. In two cases there was old heart-disease, with recent pericarditis in one; in the other two there were recent basic systolic murmurs, apparently of endocardial origin, which, in one case, disappeared after recovery. The day of the disease was reckoned in two of these cases from the first symptom, which was well marked, and which was succeeded on the second and third days by articular pain; in another case there had been a long indefinite period of slight ill-health before the articular pain set in suddenly; the disease is dated from the first articular pain; in the other case (a relapse), the articular pain and high febrile symptoms set in suddenly and simultaneously; for some days previously, however, the pulse had been rising. The disease is calculated from the day of sudden increase.

A fifth case of acute rheumatism is, for certain points, occasionally referred to.

In all these cases, in addition to the examination of the urine, and of the cardiac and articular symptoms, an accurate thermometrical investigation was undertaken. After the termination of the case, all these particulars were tabulated. The tables reach to such an extent that it has been found impossible to publish them, and I have therefore given only the conclusions. It is with considerable reluctance that I venture to publish only the average results, but it may be safely assumed, that every endeavour has been made to present them as accurately as possible.*

The relation of the urinary excretions to the weight and height of the individual has been left out of account (although some data exist for calculating these), as not necessarily connected with the matter in hand, and for the same reasons the thermometrical observations are omitted.

In three cases no treatment was adopted for the first twenty-four hours in order to see the exact condition of the urine; afterwards liquor potassæ was given in ʒss doses in simple water, and was pushed to the extent of ʒiij. to ʒvj. in twenty-four hours.

1. *The water of the urine.* (Normal average in 24 hours, 40 fluid ounces.) —Average before treatment in four observations on the 2nd, 3rd, 4th, and

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* The liquor potassæ was the preparation ordered in the London Pharmacopœia (6·7 per cent. of pure potash). Unless the contrary is stated, the diet was the hospital low diet—viz., in twenty-four hours, bread ½ lb., milk O¼, barley water Oj. No difficulty was found in collecting the whole quantity of urine passed in twenty-four hours, except sometimes in the female. The solids were determined by evaporation; the sulphuric and phosphoric acids by baryta; the chlorine by incineration at a very low temperature, dissolving the chloride of sodium in water, and precipitating and weighing as chloride of silver. The uric acid was thrown down by hydrochloric acid, and in some, though not in all cases, was dissolved in potash and precipitated by acetic acid. At the time these observations were made, Liebig's method of determining the urea and chlorine had not been made known.

5th days, diet low, water ad libitum, = 24 fluid oz. in 24 hours; the highest amount being 31½ oz.

Average during treatment (ʒiv. to ʒvj. of liquor potassæ being taken in each 24 hours; no other treatment; in 16 observations on the 5—10 days, = 34 ounces.

Average in 2 cases on the three succeeding days (11th to 13th inclusive). 30 ounces. In a third case, on the 8th, 9th, and 10th day, = 22 ounces. In a fourth case the urine was lost.

Conclusion.—By reference to the individual cases, as well as from the averages, it appears that the urine was slightly increased in quantity, and that this increase (10 ounces in 24 hours) was attributable to the medicine, and not to improvement in the disease.

2. *The urinary solids.* (Becquerel's average for men = 571 grains* — in 24 hours.)

Case 1.

No determination before treatment. From the 5th to the 8th days (inclusive), ℥iiss. of liquor potassæ being taken in this time, 2786·28 grains were passed, or 696·57 in each 24 hours. On the 12th and 13th days, the disease being well, no medicine, diet a little better, 404·57 and 391 05 grains were passed.

Case 2.

No determination before treatment. From the 5th to the 10th days, inclusive, (6 days), ℥ij. ʒvij. of liquor potassæ being taken, 5777·22 grains were passed, or 962·87 in each 24 hours. In the 6 following days no medicine; disease improving, but by no means well; diet same; 3436·34 grains were passed, or 572·72 in each 24 hours. On the 19th day, ʒiv. of liquor potassæ being taken, 947·91 grains were passed.

Case 3.

Before treatment, on the 2nd and 3rd days 713·711 grains were passed daily. On the 4th and 6th days, the urine of the 5th being lost (liquor potassæ ʒiij., vin. sem. colch. ʒj. in each 24 hours; diet same; articular symptoms much better), 1071·67 and 993·7 grains were passed. On the 8th day no medicine; disease well; 697·81 grains were passed.

Case 4.—(*Female.*)

Before treatment, on the 5th day, 469·5 grains were passed. On the 6th, 7th, and 11th days, ℥j. ʒiss. of liquor potassæ being taken, 1676 04 grains were passed, or 558·68 grains in each 24 hours. On the 23rd day, convalescence; good diet; no medicine; 364·32 grains were passed. In this case the urine of the intermediate days was partly lost with the motions.

Case 5.

During convalescence, ʒiss. of liquor potassæ being given, the solids augmented by 56 grains in 24 hours.

Conclusion.—In cases 3 and 4 the solids were above the average before treatment;† they augmented decidedly under the use of the potash. In case 1 no conclusion is possible, as the solids were not determined before treatment, and afterwards; the cessation of symptoms and the disuse of the remedy took place at the same time. In case 2 the solids were greatly increased; and that this was owing in part to the potash, is evident from the fact that when the medicine was left off, an average fall of 400 grains in the amount of the solids, in 24 hours, took place; the articular symptoms and the fever were, however, still severe, so that the diminution must be attributed to the withdrawal of the potash, and not to improvement in the disease.

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* This is usually considered to be much under the real amount, which may be taken as 650 to 750 grains for adult men on good diet.

† In reasoning on these points, it must be remembered that, in acute diseases, the influence of food, which exerts so great an effect on the urinary solids, is completely excluded; the whole amount is derived from tissue-metamorphosis.

It may therefore be said, that of the two possible factors, the disease (to use this well-understood term for the sum of the abnormal processes going on in the body) and the remedy, the increase in the urinary solids was due to both.*

3. *The Urea.*—No experiments were made, but it was noted in cases 3 and 4, that nitrate of urea crystallized at once when nitric acid was added to the unconcentrated urine; as there was at this time an increase of organic solids, it is probable that the urea was in excess.

4. *The Creatine, creatinine, &c.*—No experiments were made.

5. *The Constituents containing Sulphur.* (Ronald). In case 1, before treatment, on the 4th day, besides 52½ grains of sulphuric acid, 5½ grains of unoxidized sulphur were passing off in 24 hours. Whether this was increased by liquor potassæ, was not determined in any of these cases; but as the sulphur containing ingredients of the urine were found to be increased, in an analogous case, by the use of the bicarbonate of potash, it may be inferred that the liquor potassæ had the same effect.†

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* I may mention here, that in a chronic non-rheumatic case in which the exact weight of the body was known, and in which the ingesta were unaltered, a much greater loss of weight was found to occur when liquor potassæ had been administered than could be accounted for by the increased secretion of urinary solids which was found to take place. In this case the intestines and the skin were unaffected, and it appeared a fair inference that the loss of weight was owing in part to heightened pulmonary functions; that is to say, that the action of the potash was not confined to increasing the urinary excretion, but that the pulmonary exhalation was augmented. No direct experiments were made, and this inference was drawn simply from the great loss of weight, which was evidently due to the potash, and which was certainly not accounted for by the urine, perspiration, or intestinal excreta.

† In the case referred to, a man aged 34 was admitted with affection of the knee and ankle joints, which it was impossible to distinguish from subacute rheumatic arthritis by any symptoms connected with the joints themselves. But there was no pyrexia (temperature normal); the urinary solids passed in 24 hours were in small amount; the sulphuric acid was not increased, and the chlorine was abundant; the uric acid was deficient in the urine, and was abundant in the blood, as in gout (Garrod). The following table shows the action of bicarbonate of potash on the urine.

Calculated for 24 hours.

	Quantity in ozs.	Solids.	Uric Acid.	Soluble Salts.	Sulphuric Acid	Unoxidized Sulphur.	Remarks.
In 11 hours, no medicine was taken.	23¼	363·171	0·790	54·36	21·460	1·796	
In the succeeding 13 hours ʒiv. of bicarbonate of potash were taken.	74	847·12	....	273·4	27·5	7·002	The urine was highly alkaline, and contained a great quantity of carbonate or bicarbonate.

The bicarbonate of potash, therefore, had the following action: it passed off very rapidly, and in part as carbonate or bicarbonate, with the urine, and therefore increased the amount of

6. *The Uric Acid.*—(Normal average, 8 grains in 24 hours.)

Case 1.

Before treatment, on the 4th day	8·851 grains.
On the 5th day, (ʒvss. of liq. pot.)	9·51 „
On the 6th day, (ʒv. of liq. pot.)	9·975 „
On the 7th day, (ʒv of liq. pot.), articular symptoms almost gone	2·013 „
On the 8th day, (ʒv. of liq. pot.)	·78 „

As in other cases, liquor potassæ was not found to diminish the uric acid; and as the diminution in the above case coincided with decline of the symptoms, it is to be inferred that the effect of the potash on the uric acid was not marked.

Case 2.

No determination before treatment. From the 5th to the 10th day (℥ij. ʒvij. of liq. pot.), 12·694 grains were secreted in each 24 hours. From the 12th to 16th day (no medicine, disease improving), there were passed in each 24 hours 8·154 grains. The potash therefore caused no decrease; it may have caused an increase; but from the facts of Case 1, it is fair to conclude that the uric acid was in excess from the morbid processes, and not from the remedy.

Case 3.

Before treatment, on the 3rd day	4·439 grains.
On the 6th day, (liq. pot. ʒiij., vin. sem. colch. ʒj., being taken on the 4th, 5th, and 6th; symptoms better)	7·632 „
On the 9th day (no medicine on the 8th and 9th)	4·830 „

There appeared here to be a decided rise under the use of liquor potassæ and colchicum; but considering the known action of colchicum (Chelius, Maclagan), the augmentation may fairly be ascribed to it.

Case 4.—(*Female.*)

Before treatment (5th day)	6·772 grains.
On the 6th day (ʒv. of liq. pot.)	9·200 „
On the 7th day (ʒv. of liq. pot.)	9·200 „
On the 11th day (no medicine on 10th; ʒiss. of liq. pot. on 11th)	8.59 „
On the 23rd day (convalescence; no medicine)	1·692 „
On the 45th day (good health; good diet)	1·320 „

During the use of the potash there was a slight increase, but whether this was owing to the remedy or to the disease, is doubtful.

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the soluble salts; it augmented also the water by 50 ounces (the calculation was made for 24 hours), the organic solids by 264 grains, the sulphuric acid by 7 grains, and the sulphur by more than 5 grains.

The action of the bicarbonate of potash appears so far to differ from that of the liquor potassæ, that some considerable portion passes off unchanged in the urine; whenever this is the case, the urine is rendered alkaline. This passage occurs with such rapidity, that, if it is wished to keep up the increased alkalinity of the blood, the salt must be given very frequently (every hour or so); if given only every four hours, in less than two it is excreted, so that for two hours the blood is unacted upon. The facts now given show how erroneous is the statement Durand Fardel has lately published respecting the action of the alkaline (bicarbonate of soda) Vichy waters. Because some of the salt passes out unchanged, he concludes that all does so, and that it is simply a poison, rapidly excreted with the urine. The same reasoning might be applied to the bicarbonate of potash; because some part of the salt is excreted unchanged in the urine, we might argue that all is so, and that the bicarbonate produces no other effect on the system. Chemical analysis shows, however, that besides the excretion of the salt, the organic solids, the sulphur, and the sulphuric acid, are all increased.

Conclusion.—The uric acid is in most cases in excess in rheumatic urine, but is not always so. Liquor potassæ may cause some increase, but the influence cannot be great, and certainly this medicine will not prevent the fall in the amount of uric acid which occurs when the disease is passing off. Liquor potassæ and colchicum together cause an increase, but this is perhaps due to the latter medicine.

7. *The Hippuric, Oxalic, and other Acids.*—No experiments were made.

8. *The Soluble Salts.*—The total amount of the soluble salts, determined by incineration and dissolving, was ascertained only on three occasions. In case 1, on the 4th day before treatment, 191·614 grains, which were made up almost entirely of sulphates and phosphates, were passed in 24 hours. On the 7th day (ʒv. of liq. pot.) 123·782 grains were passed, composed entirely of sulphates and phosphates. In Case 2, on the 8th day (ʒvj. of liq. pot.) 151·812 grains were passed, composed almost entirely of sulphates and phosphates.

The changes in the soluble salts will be best seen by considering the changes in the chlorine, sulphuric, and phosphoric acids.

(*a*) *Chlorine.* (Healthy average, 90 grains in 24 hours.) The diminution of the chlorides in pneumonia, and sometimes in pleurisy, has attracted much attention (Redtenbacher, Beale); it appears, however, to be equally common in rheumatic fever.

In Case 1, on the 4th day (no treatment), there was an extremely scanty precipitate given by the direct addition of nitrate of silver and nitric acid, to the urine; on the following day there was none whatever; on the succeeding day, traces; on the next day, none; and it was not, in fact, till the 9th day, that there was any quantity; it returned then from day to day; on the 12th day (the diet being only altered by the addition of a light pudding), 22·264 grains of chlorine were passed in 24 hours; and on the following day there was evidently a still greater quantity. That of the three possible factors, the withdrawal of food, the presence of the disease, and of the remedy, the absence of the chlorine was owing to the second, is evident from the fact that the chlorine began to return before the diet was changed; and that in this, and in the other cases, and also in health and in all cases of disease yet examined, the liquor potassæ has never been known to have the effect of diminishing the quantity of chlorine.

In Case 2, on the 7th day, only 4·665 grains of chlorine were excreted in 24 hours; on the following day there was still less; on the 18th day, when the quantity was next determined, nearly 60 grains were passed, although the diet had been only very slightly altered. So also in Case 4, on the 14th day (when the point was first examined, and when the articular symptoms were still severe), there was no chlorine, and it remained absent till about the 19th day, when the disease was passing off. In Case 3 no observation was made.

These facts show that in acute rheumatism there is in some cases (perhaps in all) a diminution in the amount of chlorine excreted during the height of the disease, greater than can be accounted for by the absence of food. It is also evident that liquor potassæ (in the doses above given) does not cause the reappearance of the chlorine.*

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* It is of course an obvious suggestion, that the chlorides may in part pass off with the copious perspirations, which in all these cases were very great. Or they may be retained in the system, or even possibly be decomposed.

(*b.*) *Sulphuric acid* (healthy average, 30 grains in 24 hours (Vogel), 17 grains (Becquerel), 24 grains (Author.)

Case 1.

Before treatment (4th day)	52·668	grains.
5th and 6th days, in each 24 hours (ʒv. of liq. pot. on each)	55·363	,,
7th, 8th, and 9th days (ʒv. of liq. pot. on each), symptoms much better	43·644	
10th and 11th days (no medicine; convalescence)	32·237	
12th and 13th days (no medicine; better diet)	24·147	

Case 2.

No observation before treatment. There were excreted in each 24 hours, of sulphuric acid, from the 5th to the 10th day (144 hours, ℥iij. ʒv. of liq. pot. being taken)	56.449	grains.
From the 11th to the 18th, in each 24 hours (no medicine; symptoms better; not well)	39·903	
On the 19th day (ʒiv. of liq. pot., symptoms of rheumatism well)	44·555	
On the 29th and 30th days (no medicine; symptoms well; good diet)	34·164	
From the 31st to the 34th (℥j. ʒvj. of liq. pot. being given for experiment; the rheumatic symptoms being well) in each 24 hours	52·035	

Case 3.

On the 2nd and 3rd day, in each 24 hours (no medicine)	32·267	grains.
On the 4th and 6th days (liq. pot. ʒiij. vin. sem. colch. ʒj.)	40·281	,,
From the 8th to the 14th day, in each 24 hours (no medicine; convalescence)	23·67	,,

Case 4.—(*Female.*)

On 5th day (no medicine)	26·144	grains.
On 6th and 7th days (ʒv. of liq. pot. on each)	37·625	,,
On 11th day (ʒiss. of liq. pot.; symptoms better)	25·008	,,
On 23rd day (convalescence; no medicine)	6·412	,,
On 45th day (good diet)	5·40	,,

Conclusion.—In rheumatic fever the sulphuric acid is greatly augmented; it falls to the normal amount or below it, when the severe symptoms have ceased.* Liquor potassæ increases the amount, and delays, but does not entirely prevent, the fall which occurs during commencing convalescence. The same effect is produced by potash and colchicum.

(*c.*) *Phosphoric Acid.*—(Healthy average, 55 grains. Gruner.) The phosphoric acid was not determined before treatment, and only on three occasions afterwards. In Case 1, on the 7th day (liq. pot. ʒv.) 24·613 grains were passed; on the 12th day (no medicine, better diet), 22·264 grains. On the 13th day (no medicine), 8·73 grains.

Conclusion.—The facts are not sufficient to warrant any definite opinion, but from the low figure of the phosphoric acid on the 13th day, the 24 grains passed on the 7th day (when no food was taken) are, perhaps, an excess of what would have been furnished by the tissue metamorphosis alone, of the healthy system. The excess might be due to the disease or to the remedy.

9. *Insoluble Salts.*—No observations were made.

10. *Foreign Matters.*—Albumen was present in small amount in two

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* From Case 5 it was ascertained that the sulphuric acid was in as great a quantity in the urina sanguinis as in the urina cibi; the amount of food was, however, small.

cases; in one before and during treatment; in one during treatment. In both cases it disappeared before the liquor potassæ was discontinued, and was manifestly unaffected by it.

11. *The Acidity, Deposit, &c.*—The urine was highly acid before and during treatment, but the exact amount was not ascertained; it was never alkaline. The colour became paler (independent of dilution) under the influence of the liquor potassæ. The deposit (amorphous, deep-coloured lithates) was not affected by the medicine except as far as the colour was concerned.

The following summary expresses in general terms the action of liquor potassæ on the urine of 24 hours:

Urinary Constituents.	Condition In Rheumatic Fever.	Effect produced by Liquor Potassæ in large doses.
Water	Greatly diminished . .	Slightly increased; (over rheumatic average.)
Solids	Increased	Still more increased.
Urea	Probably increased . .	Probably increased.
Uric Acid	Increased	If affected at all, slightly increased.
Creatine, &c.	Undetermined.	
Sulphur containing comp.	In considerable quantity	Probably increased.
Chlorine	Diminished	Unaffected.
Sulphuric Acid	Greatly increased . . .	Still more increased.
Phosphoric Acid	In some quantity . . .	Uncertain.
Bases	Undetermined.	
Insoluble Salts	Undetermined.	

The urine in rheumatic fever appears from these cases to have peculiarities which distinguish it from the urine of other fevers. It resembles the typical febrile urine in its deficiency of water, in its depth of colour, in its great per-centage of solids, and in the rapid deposit of dark urates. But apart from possible differences connected with the urea, and extractives, it differs from the febrile urine of pneumonia and specific fevers in the greater amount of the absolute excretion of solids (i.e., in 24 hours) and in the enormous excess of sulphur, and its derivatives. The excretion of sulphuric acid is far greater than in any other febrile disease which I have examined,—viz., than in small-pox, typhus and typhoid fever, scarlatina, erysipelas, pyæmia (with purulent arthritis), pleurisy, and pneumonia. In these cases the sulphuric acid has been also in excess of that which would have been formed during healthy tissue metamorphosis, except in some cases of pneumonia in which the sulphuric acid (in common with other urinary ingredients) has been retained in the system during hepatization, and been poured out afterwards during resolution.

The excess of sulphuric acid in the rheumatic urine is not due to an excess of febrile action in this disease, over the other fevers just enumerated. In cases of typhoid fever and scarlatina, the temperature has been higher than in rheumatic fever; and yet the amount of sulphuric acid passed in 24 hours has not reached to half the quantity.

The sulphuric acid is not, then, in any close proportion to the temperature. As the temperature is usually considered to be a correct indication of the rapidity of tissue-metamorphosis in febrile diseases, it follows that rheumatic fever is an exception to the rule, and that the sulphuric acid is in excess of what would have been predicated from the amount of fever.

It appears, therefore, a fair inference that in rheumatic fever there is a source of sulphuric acid, independent of the augmented disintegration of tissues, as measured by the heightened temperature, and it may perhaps be conjectured that chemical analysis will hereafter demonstrate the existence in the blood of some compound richer in sulphur than fibrine and albumen, which during the height of the disease is rapidly disintegrating, and forming, probably among other products, sulphuric acid.

The effect of liquor potassæ, of the bicarbonate, and perhaps of the other alkalies, is at once to aid this disintegration, and to increase the elimination of sulphuric acid, by augmenting the alkalinity of the blood. If this hypothesis be correct, the administration of alkalies in rheumatic fever would acquire a basis more rational than that usually assigned—viz., that they merely neutralize acid already formed.

With respect to the efficacy of liquor potassæ in rheumatic fever, although the cases are so few in number, yet as it is unlikely that additional cases can be treated so rigidly without other medicines, and as in fact other remedies (mercurial purgatives, colchicum, opium, hot air bath, &c.) ought to be employed, I may mention the general conclusions which may be deduced from these four cases.

No symptom was immediately affected, except the pulse; this was generally, but not always lowered, and sank, although the temperature continued high, to 80, 70, and even lower. The febrile heat (as measured in the mouth), the articular pains, and the perspirations, were not affected, except in so far *that the duration of the disease* was shortened. The effect on cardiac complication was uncertain; in one case pericarditis came on, but there was a strong suspicion that it had actually begun before the medicine was commenced; in two cases basic systolic murmurs appeared, in one case before, in the other (a man), decidedly after the potash was commenced. This murmur, however, disappeared during convalescence.

The duration of the disease was in three cases short, although the severity of the early symptoms led to the belief that it would be obstinate and long continued. The first case lasted scarcely a week; the second, 18 days; and the third (relapse) about 7 days. In the 4th case (old and recent heart-complication, pericarditis) the duration was greater, and the patient was not convalescent till the 23rd day. The average of the whole was 13·75 days from the first symptom; but as an average of 3·25 days occurred before treatment, the actual period from commencement of treatment to perfect freedom from joint-pain and fever, was 10½ days. This result (if it occurred in all cases) would certainly be favourable, but it is well known that other observers have obtained equally fortunate results from very different treatment; so that the superiority of the treatment by liquor potassæ, *per se*, cannot be held to be sufficiently proved.

A great disadvantage in liquor potassæ is its nauseous taste, and frequently also, after a time, if it be given in large doses (and this is necessary), the stomach does not tolerate it well.

PART FOURTH.

Chronicle of Medical Science.*

ANNALS OF PHYSIOLOGY.†

By HERMANN WEBER, M.D.,
Physician to the German Hospital.

I.—FOOD AND DIGESTION.

1. *The Ligature of the Pancreatic Duct.* By Professor HERBERT, (Zeitschrift für rat. Med., von Henle und Pfeuffer. Vol. iii. Heft 3, p. 389.)
2. *The Amount of Nitrogenous and Non-Nitrogenous Elements in various (English) Dietaries.* By Dr. BENEKE, (Archiv für phys. Heilkunde von Vierordt. 1853. Heft 3, p. 409.)

1. HERBERT made his experiments (tying the pancreatic duct) principally for the purpose of proving the incorrectness of the observation of Bernard, corroborated

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[*] This portion of the Review is occupied principally with the abstracts of Journal-articles and of short treatises. In each Quarterly Report on Medicine, Surgery, Midwifery, and Medical Jurisprudence, the journals received during three months will be analysed; and most, if not all, the papers in the Foreign periodicals will receive some notice. The British journals are so universally in the hands of our readers, that it is scarcely necessary to do more than glean from them here and there a paper of peculiar interest. The Report on Midwifery will not be commenced till April. Physiology, Micrology, Chemistry, and Materia Medica are discussed in half-yearly and yearly reports.

The space which can be allotted to these Reports is so small, compared with the amount of matter to be analysed, that no pretensions can be made to anything like a complete record of each subject. A full and judicious selection is all that is aimed at. During the months of September, October, and November, 1853, in addition to the British journals, the following periodicals were received:—

GERMAN.

1. Archiv für path. Anat. von R. Virchow. Band v. Heft 4, and Band vi. Heft 1.
2. Archiv des Vereins für gemeinschaftliche Arbeiten; Band i. Heft 1 and 2.
3. Archiv für phys. Heilkunde von Vierordt. 1853. Heft 3.
4. Archiv für phys. und path. Chemie, von F. Heller. N. F. Band ii. Heft 6.
5. Archiv für Anat. und Phys. von J. Müller. 1853. Heft 3 and 4.
6. Zeitschrift für rat. Med. von Henle und Pfeuffer. Band iii. Heft 3.
7. Zeitschrift (Henke's) für die Staatsarneikunde, von Behrend. 1853. Heft 3.
8. Zeitschrift der K. K. Gesells. der Aertze zu Wien, von Helra. (August and September.)
9. Prag Vierteljährschrift für Praktische Heilkunde. 1853. Band iii. and iv.
10. Verhandl. der Phys. Med. Gesell. der Aertze zu Wurzburg. Band iv. Heft 1.
11. Schmidt's Jahrbucher der Ges. Med. 1853. No. 9, 10, and 11.

FRENCH.

12. Archives Générales de Médecine. 1853. September, October, and November.
13. L'Union Médicale. September, October, and November.

ITALIAN.

14. Bulletino delle Scienze Mediche. Bologna. June, 1853.

AMERICAN.

15. The American Journal of the Medical Sciences. October. 1853.
16. The Philadelphia Medical Examiner. September, October. 1853.

[†] The Annals of Physiology were commenced in July, 1853, by Mr. Gray, but indisposition has unfortunately compelled that gentleman to discontinue their preparation.

by the commission of the Academy of Paris (Magendie, Dumas, Milne-Edwards*) concerning the function of the pancreatic juice. Bernard ascribed to it, as is well known, the property of emulsifying the fatty matter, and considered as its chief action, the power of effecting such a change in the oleaginous constituents of the chyme as to make them fit for absorption Herbert tied the pancreatic duct in two rabbits, after they had been starved for several days; on the day after the operation, the rabbits were fed with milk and bread a few hours before they were killed. At the post-mortem, the thoracic duct was found to contain a considerable quantity of a bluish-white fluid, the lymphatic vessels of the mesentery were completely filled, and of a milky colour The same result was obtained by feeding a rabbit after the operation with roasted bacon, potatoes, and water. The further observation of Bernard and the French commission—that the lymphatic vessels of the mesentery do not exhibit the milk-white colour above the entrance of the pancreatic duct into the duodenum, that they commence to do so only two centimeters below this (0 787 inches)—is likewise not corroborated by Herbert; the latter having always found the milky colour of the vessels half an inch above the opening of the duct The conclusions of Lenz and others are therefore confirmed.

2 Dr BENEKE has written a very important paper on the amount of food allowed in various institutions in London, and has based some interesting calculations on these data. He first gives an account of each institution, to show its object and the class of individuals residing in it This we pass over, as containing only familiar matter, and proceed to the tables, which are so valuable that we shall give them entire

1. EDUCATIONAL AND INVALID ESTABLISHMENT.

Amount of Food consumed by each Person weekly, in ounces.

Place	Age	Sex	Meat	Bread	Potatoes	Fresh Greens	Sugar	Flour	Fat	Milk	Tea	Coffee	Cocoa	Beer	Cheese	Rice	Occupation, &c
Royal Military Asylum, Chelsea	5 14	M	41	105	26		4	6	1	105			3½			5	8½ hours sleep 6 hours school.
Royal Hospital, Chelsea	Mean 68	M	60	112	112		8¼	7	4	30	1¼		5¼	140	8		Ad libitum.
Royal Hospital, Greenwich	Mean 70	M	58	112	70	7	8	14	12	90	1½		6	280			Ad libitum
Royal Navigation School, Greenwich	11 18	M	44	105	42	6¼	8¾	6	5½	40	1¼		3½				9 hours slee 6 hours schoo
The Victory, Portsmouth	14-60	M	74	112	56		12	7			1½		7				7 hours sleep
Christ's Hospital, London	7-15	M	20	109	40			12	3½	70				70	12		10 hours slee 7 hours schoo
Refuge, Dalston	11 32	F	48	112	84		8	16	8	70	1	2			7		8½ hours sleep
London Orphan Asylum.	7-15	M & F	33	105	36		1¼	16	4½	70					7		9 10 hours slee 6½ school
Hackney Workhouse																	
1	15-60	M	15	96	26		3½	24	7½				2		2		Various.
2	15 60	M	10	100	24			34	4						3		"
1	15 60	F	15	82	36		3½	24	7½				2		2		"
2	15-60	F	10	86	24			34	4						3		"
1 2	7-15	M & F	As the women														"

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* Comptes Rendus hebdom. des Séances de l'Académie des Sciences. Tom xxvii, p. 249, à p 288.

2. PRISONS.

Place.	Age.	Sex.	Meat.	Bread.	Potatoes.	Greens.	Sugar.	Flour.	Fat.	Milk.	Tea.	Coffee.	Cocoa.	Beer.	Cheese.	Rice.	Occupation, &c.
10. Pentonville Prison	16–60	M	28	140	102	...	10	10½	.	14	..	.	5¼	.	...	.	8 hours sleep.
11. Bridewell Prison																	
Over 2 months .	16–60	M	24	140	32	..	8	10½	.	14	.	..	7	.	..	14	10 hours sleep.
Under 2 months	16–60	M	12	140	16	...	.	19½	.	.	..	.	.	..	..	7	„
Over 2 months .	16–60	F	18	140	..	..	8	12	..	14	..	..	7	..	.	9	„
Under 2 months	16–60	F	12	112			..	20	.	...	.	.		...	.	6	„
.. Over 2 months .	7–16	M&F	12	112	16		..	23	..	..	..	..	..	..	..	6	„
Under 2 months	7–16	M&F	6	112	.	.	.	23	..		..	.	..	..	..	3	„
12. Stirling Castle, Portsmouth .	14–60	M	38	161	112	..	..	10½	..	..	..	...	5¼	..	.	.	8½ hours sleep
13. Norwich Castle. .	14–60	M	..	..	...	...				.		..	.	.			8 hours sleep.
Third Class . .	...	..	6	168	70	2	.	21		.	..	.	.	.	.	2	2-6 weeks hard labour
Fourth Class . .	...	...	12	168	40	3	.	21	.			..	...			3	6-12 weeks hard labour.
Fifth Class . .	...	...	10	126	121	3	2¼	10½		..	.		.	2½	..	3	Over 3 months hard labour
14. Millbank Prison .	14–70	M	31	154	118	.	7	14		14		..	1½	.	.	2	9 hours sleep.
	14–70	F	26	140	62	.	7	14	.	14	.	..	1½	...	..	2	

3. HOSPITALS.

Place.	Age.	Sex.	Meat.	Bread.	Potatoes.	Greens.	Sugar.	Flour.	Fat.	Milk.	Tea.	Coffee.	Cocoa.	Beer.	Cheese.	Rice.	Occupation, &c.
15. Hanwell Lunatic Asylum . . .	15–70	M	21	96	60	10	3½	16	3	20	..	.	3½	140	14	3	Various.
	15–70	F	21	80	60	10	3½	16	6½	20	1¼		3½	70	..	3	„
16. Middlesex Hospital	...	M&F	28	84	56	..	...	10½	.	70		.	..	.	.	...	Convalescents.
17. Bartholomew's Hospital . .	...	M&F	28	84	56	..	..	7	5¼	70		.	..	140	..	.	„
18. Hospital for Consumption . .	..	M&F	28	84	37	.	..	1	...	96	.	..	7	...	...	..	„
19. St. George's Hospital	..	M&F	42	84	56	.	.	10½	7	70	.	.	..	140		...	„
20. Westminster Hospital	...	M&F	56	98	84	..	..	21	..	70			..	...	...	...	„
21. German Hospital, Dalston . . .	...	M&F	28	84	56	.	..	10½	7	140		.	..	..	.	...	„
22. Woolwich (Military) Hospital .	...	M&F	38	84	112	..	3½	16	..	70	...	...	..	...	...	5¼	„
23. Royal Sea-Bathing Infirmary .	10–40	M&F	52	120	52	...	...	10	3	80	..	.	2	280	.	9	„
	4–10	M&F	38	96	52	..	...	10	2	80	...	..	2	140	..	9	„
24. Metropolitan Establishment, Margate . .	5–10	M&F	20	70	40	..	..	..	4	70		..	..	25	...	12	„
	10–16	M&F	30	98	80	...	7	21	6	35	2	.	..	50	...	..	„
25. Chateau Bellevue, Margate . . .	16–40	M&F	41	98	80	...	3½	10	6	70	1½	..	...	140	...	3	„

Dr. Beneke then proceeds to calculate the various amounts of water in these articles of food. He uses for this purpose the data given by Mülder, Liebig, and Frerichs, for meat; those of Liebig, Horsford, and Krocker, for bread; those of Mülder, Horsford, and Krocker for potatoes, &c. In several cases (in bread, for example) he has also ascertained by direct experiment, that the quantity of water mentioned by these authorities was the same in the English food. He then reckons the relative quantity of nitrogenous and non-nitrogenous food in the water-free substances. Fat he reckons, and chiefly from Liebig's data, as equal to 2·4 parts of starch. The bread is reckoned as containing in every pound [illegible] of a pound of flour (Liebig). The following table shows at once the data on which he bases the calculations:

Food.	Nitrogenous Proportion.	Non-nitrogenous Proportion. (Reckoned as Starch.)	Water.
Meat	1	1·7	70 per cent.
Wheat-flour	1	4·6	14 „ „
Potatoes	1	8·6	75 „ „
Sugar	0	1	— „ „
Fat	0	2·4	— „ „
Milk	1	·3	87 „ „
Cheese	1	2·59	32 „ „
Rice	1	13·7	15 „ „

The small quantities of tea, cocoa, and beer are disregarded. Calculating then from these data, Dr. Beneke finds the following weekly consumption of nitrogenous and non-nitrogenous food in these various institutions; the amounts are expressed in ounces avoirdupois:

Place.	Nitrogenous	Non-nitrogenous. (Reckoned as Starch.)	Proportion of Nitrogenous to Non-nitrogenous.
1 Royal Military Asylum	22·80 ozs.	96·80 ozs.	As 1 is to 4·2
2. Royal Hospital at Chelsea	26·42 „	127·91 „	„ 1 „ 4·8
3. Royal Hospital, Greenwich	26·58 „	143·28 „	„ 1 „ 5·4
4. Royal Navigation School, Greenwich	20·80 „	105·69 „	„ 1 „ 5·08
5 The Victory, Portsmouth	24·06 „	104·78 „	„ 1 „ 1·43
6. Christ's Hospital, London	22·53 „	101·66 „	„ 1 „ 1·45
7. Refuge, Dalston	26·83 „	146·04 „	„ 1 „ 1·54
8. London Orphan Asylum	23·20 „	106·15 „	„ 1 „ 1·46
9. Hackney Workhouse, I. a	18·14 „	103·37 „	„ 1 „ 5·7
„ „ b	19·48 „	97·45 „	„ 1 „ 5·00
„ „ II. a	16·45 „	95·58 „	„ 1 „ 5·8
„ „ b	17·81 „	89·78 „	„ 1 „ 5·04
10. Pentonville Prison	24·56 „	123·91 „	„ 1 „ 5·04
11. Bridewell Prison	23·06 „	116·61 „	„ 1 „ 5·06
„ „	21·87 „	102·14 „	„ 1 „ 4·67
„ „	21·53 „	105·80 „	„ 1 „ 4·9
„ „	18·04 „	82·51 „	„ 1 „ 4·57
„ „	18·89 „	88·22 „	„ 1 „ 4·65
„ „	17·65 „	81·13 „	„ 1 „ 4·60
12. Sterling Castle, Portsmouth	28·03 „	128·41 „	„ 1 „ 4·50
13. Norwich Castle	25·95 „	125·80 „	„ 1 „ 5·09

Places.	Nitrogenous.	Non-nitrogenous. (Reckoned as Starch.)	Proportion of Nitrogenous to Non-nitrogenous.
13. Norwich Castle	25·90 ,,	121·10 ,,	,, 1 ,, 4·9
,, ,, ...	22·73 ,,	115·89 ,,	,, 1 ,, 4·8
14. Millbank Prison	27·66 ,,	136·72 ,,	,, 1 ,, 4·8
,, ,, ...	23·97 ,,	115·64 ,,	,, 1 ,, 5·6
15. Hanwell Lunatic Asylum . . .	21·32 ,,	103·63 ,,	,, 1 ,, 4·8
,, ,, ,, . . .	17·51 ,,	97·59 ,,	,, 1 ,, 5·6
16. Middlesex Hospital	18·51 ,,	78·40 ,,	,, 1 ,, 4·23
17. Bartholomew's Hospital . . .	17·98 ,,	88·52 ,,	,, 1 ,, 4·9
18. Hospital for Consumption . . .	17·40 ,,	69·97 ,,	,, 1 ,, 4·02
19. St. George's Hospital	20·07 ,,	97·83 ,,	,, 1 ,, 4·8
20. Westminster Hospital	25·55 ,,	104·74 ,,	,, 1 ,, 4·10
21. German Hospital	20·78 ,,	102·02 ,,	,, 1 ,, 4·9
22. Woolwich Hospital	22·20 ,,	104·41 ,,	,, 1 ,, 4·2
23. Royal Sea-Bathing Infirmary, Margate	26·23 ,,	116·80 ,,	,, 1 ,, 4·5
,, ,, ,,	21·77 ,,	98·57 ,,	,, 1 ,, 4·52
,, ,, ,,	14·59 ,,	77·21 ,,	,, 1 ,, 5·3
24. Metropolitan Establishment, Margate	21·94 ,,	119·12 ,,	,, 1 ,, 5·4
25. Château Bellevue, Margate . .	22·30 ,,	113·60 ,,	,, 1 ,, 5·05

The proportions of nitrogenous and non-nitrogenous food are nearly the same. In the Hospital for Consumption there is a relative excess of nitrogenous substances, and the whole amount of food is small. Dr. Beneke believes that this diet is the most properly calculated, and is most to be recommended. In Hackney Workhouse the non-nitrogenous food is in relative excess, but even here, the amount of nitrogenous food is above the allowance of the Hospital for Consumption, and is probably too great.

The mean proportion may be cited as 1 to 5. The proportion given by Frerichs, as deduced from calculation, is 1 to 7. Frerichs reckoned that an adult man should consume in 24 hours, 2·17 ounces av. of nitrogenous, and 15·54 ounces of non-nitrogenous food: this would give weekly 15·19 ounces and 108·78 ounces, respectively. It will be seen that this theoretical calculation has ranked the quantity of the nitrogenous food too low, and the non-nitrogenous too high, if the practice of these institutions be taken to be the right rule. Is, however, the amount of nitrogenous food too great in these cases? It would appear also, from these tables, that the highest amount of nitrogenous food is not always commensurate with the quantity of meat allowed; the bread furnishes a large proportion of azotized aliment.

II. Respiration and Circulation.

1. *Mechanism of the Respiration and Circulation in a state of health and disease,* By F. C. Donders. (Zeitsch. fur rat. Med. von Henle und Pfeuffer. 1853. Heft, iii. p. 287.)
2 *On the Irritability of the Ciliary Epithelium.* By Rudolph Virchow. (Archiv für Path. Anat. von Virchow. Vol. vi. p. 133.)
3. *Experimental Researches applied to Physiology and Pathology.* By Dr. Brown Sequard. (Philadelphian Medical Examiner, August, 1853.)

4. *On the Topography of the Temperature of the Body.* By Dr. Fick. (Müller's Archiv. 1853. Heft 3.)

1. The most important points elucidated by Dr. Donders' experiments are as follows:—

The force of the *elasticity of the lungs*, as seen by their contraction (erroneously called "collapse") soon after the thorax is laid open, has been measured by means of a manometer placed in the trachea. As the standard in healthy men, after a normal expiration, Donders adopts the number of 80 millimeters of water. (1$^{\text{mm}}$=0·03937 inch). This number is, of course, much increased by a deep inspiration, amounting even to 243$^{\text{mm}}$ of water, or to 18$^{\text{mm}}$ of mercury. During life to this force of the elasticity that of the *tonus* of the contractile fibres under the influence of the nervous system must be added. Donders calculates the latter to be equal to 20$^{\text{mm}}$, which would raise the power of resistance of the human lung to 100$^{\text{mm}}$ of water (or 7½$^{\text{mm}}$ of mercury.) By the usual inspiration this power is increased to about 9$^{\text{mm}}$ of mercury, and by the deepest inspiration to 30$^{\text{mm}}$ mercury. This force must be overcome during inspiration; but it promotes, on the other hand, the act of expiration. Donders does not, however, agree with Kiwisch, who considered the lungs to be the only active organs during expiration. He attributes a considerable influence to the power of gravity on the walls of the thorax, to the elasticity of the latter, to the contraction of the abdominal muscles, and the increased tension of the gas in the intestinal tube pressing on the diaphragm. It is evident that in morbid states of the lungs (alteration of the elasticity, &c.), the result of these experiments must be different, and that the latter may serve as means for the diagnosis.

Concerning the influence exercised by the mechanism of respiration on *the circulation*, Donders calls particular attention to the fact, that even during the act of expiration, the heart and the great vessels in the cavity of the thorax are under a less considerable pressure than the vessels of the other parts of the body; that, therefore, the venous blood is constantly sucked into the cavity of the thorax. The difference of pressure amounts, after a normal expiration to 95$^{\text{mm}}$ (mercury), after a normal inspiration to 9$^{\text{mm}}$, after a very deep inspiration to 30$^{\text{mm}}$. These numbers indicate the force by which the venous blood is sucked in after a usual expiration and inspiration, and after a very deep inspiration. The same force must be abstracted from that by which the arterial blood is propelled from the thorax through the contraction of the heart. If, however, during forced expiration, the expulsion of the air from the lungs is prevented, the pressure of the air in the lungs on the heart and thoracic vessels may become increased to more than that of one atmosphere, showing itself by redness of the face, swelling of the veins, &c. &c. Donders alludes to the influence exercised by this diminished pressure within the cavity of the chest on the circulation in the veins, especially those of the abdominal system. Concerning the circulation of the blood through *the lungs*, Donders again remarks, "that the blood in the principal pulmonary arteries and veins is always under a lower pressure than that within the capillaries of the lungs, however the mechanism of respiration may be modified during health or disease." Even during forced inspiration, when the principal thoracic vessels are under a higher pressure than that of one atmosphere, the pressure on the capillary system of the lungs is still greater; the blood is, therefore, always propelled towards the heart. Through this arrangement the circulation through the lungs may still remain normal, while that through the other part of the body is already much deranged by abdominal respiratory pressure.

2. While hitherto no agencies were known, either mechanical, or physical, or chemical, by which the motion of the ciliary epithelium could be excited; while Purkinje and Valentin,* in their manifold experiments, had even found that all the agencies usually exciting motion in the contractile tissues seemed to destroy the motion of the cilia, Virchow has lately detected that by a solution of

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* *Purkinje et Valentin*, De phenomeno gen. et fund. motus vibratorii continui. Vratisl. 1835.

potassa ("kalilauge," the strength of the solution not mentioned), the motion of the ciliary epithelium of the human trachea is re-excited after it has completely ceased. The same effect is produced by a solution of soda, but not by that of ammonia, as the latter produces, at once, the decomposition of the cells. Virchow adds the remark, that by this observation we are entitled to consider the contractile substance of the cilia to be similar to that of Lehmann's *syntonin*, in the contractile tissue of the muscles. Though Virchow acknowledges this phenomenon to be produced by chemical action, yet he thinks that the contractile substance of the cilia exhibits also a motion peculiar to itself, and independent of the chemical "*corrosion.*" We are doubtful, however, whether the term "*irritability*" can be used until it is more fully proved that the phenomenon is not a merely chemical one.

3. Dr. Brown Sequard considers the *carbonic acid* contained in the blood to be *the cause of the beatings of the heart.* His theory is based principally on the following observations: 1. If warm-blooded animals are prevented from breathing, the beatings of the heart become more frequent for one or two minutes. 2. The hemadynamometer shows an increased energy of circulation during asphyxia. 3. All the causes which increase the formation of carbonic acid gas in the body increase the frequency of the beatings of the heart. The *rhythmical alternation* of contraction and relaxation is explained by the circumstance of the exciting cause not acting constantly with the same power. The small blood-vessels and capillaries being compressed during the muscular contraction, there is a diminution of excitation during that time; in consequence of the elasticity of the fibres, dilatation is produced as soon as the exciting cause is diminished. The fact of the heart being the only striated muscle with rhythmical movements, is explained by its possessing more blood in its capillaries (more stimulus), and having less resistance to overcome.

4. Professor L. Fuk is occupied with observations and experiments on the temperature in various organs and on various localities of the body. The twelve experiments hitherto published are performed on living dogs. The temperature is taken in the carotid (right) vena jugularis, right and left ventricle of the heart, cavity of the thorax, rectum, vagina, urethra, and brain. We shall not now enter into details, as the author himself considers his experiments only as preliminary; but we must draw attention to the interesting and almost uniform result, that no difference was found in the temperature of the right and left ventricle of the heart, as generally supposed to be the case; further, that the highest degree of warmth was always met with in the vagina and rectum (101·75° to 105·79° F., i.e., 1·125° higher than in any other organ of the body); also in the rectum of men Fuk found the temperature almost always higher than under the tongue (the mouth having been kept closed), the difference amounting in some cases to more than 3° F.

III.—Lymphatic System and Ductless Glands.

1. *On the Absorption of the Chyle from the Intestinal Tube.* By Prof. E. Bruecke.
2. *On the Origin and Course of the Lacteals in the Walls of the Intestinal Tube.* By the same author.
3. *On the Lacteals and Locomotion of the Chyle.* By the same author.

(Extracts from the Transactions of the Royal Imperial Academy of Medicine of Vienna. 1853.)

Bruecke states, as the result of his observations and experiments (on man, swine, and other mammalia), that the cylindrical epithelial cells of the mucous membrane of the intestines, through which the chyle passes on its way to the lacteal vessels, do not, as it is generally supposed, consist of a closed cavity surrounded by a complete membrane, but that this cavity is isolated from that of the intestinal tube merely by a thin layer of a mucilaginous substance. Beneke asserts also that they possess a small opening on their opposite side, through which the molecules of fat pass into the interior of the villi. Within the villi he could not detect any vessels for chyle, but merely channels (without a membrane) formed by the interstices between the elements of the tissue (blood-vessels, muscular fibres, cellular tissue). As the mechanical means for the movement of the chyle, Bruecke considers, first, the muscular contractions of the intestinal tube by which the chyle is pressed into the villi, which are in a fit state for imbibition by being distended through the pressure of the blood in their blood-vessels; secondly, when the villi are filled, their muscular fibres contract, and the fluid they contain is pressed into the channels lying between the mucous and submucous membrane. At the same time, however, a part of the fluid contained within the epithelial cells is squeezed out again into the cavity of the intestinal tube. From the lacteals within the walls of the intestines the chyle is propelled by means of the muscular contractions of the tube into the vessels of the mesentery, from whence it is pumped up, so to say, by means of the respiratory actions, into the thoracic duct. The commencement of complete lacteal vessels was recognised (in the gut of a child) in the deeper part of the mucous membrane, exhibiting a diameter of one centimillimeter (0·0003937 inch); these were seen uniting to larger vessels, which, in the submucous cellular tissue, were furnished already with many valves, with an epithelial and also with a distinct muscular layer. In the smaller ramifications (less than two centimillimeters in diameter) no valves could be detected, nor an epithelium, and also no tunica propria could be distinguished from the surrounding cellular tissue forming the adventitia. In the mucous and submucous tissue the course of the lacteals appeared independent of that of the blood-vessels, but on their passage through the muscular layer every couple of blood-vessels (an artery and a vein) was accompanied on each side by a lacteal. The chyle did not appear to enter into the vessels merely from the villi, but also from between them, and principally from between the crypts of Lieberkuhn, where it may be seen placed in the same manner as within the villi, i.e., lying freely in the interstices of the stroma. In the weasel the arrangement is similar to that in man; the lacteals, however, do not appear to be provided with valves before their entrance into the muscular layer. In rabbits a difference is observed—namely, in the whole of the wall of the intestinal tube the chyle does not appear to be conveyed in separate channels, but the adventitia of the blood-vessels, together with the continuation of that from the lacteals in the mesentery, form a sheath round the blood-vessels within the wall of the tube; in the interstices between this sheath and the blood-vessels the chyle is carried towards the origin of its own independent vessels in the mesentery. In the rabbit, therefore, during the passage through the whole of the intestinal wall, the blood is separated from the chyle merely by the thin membranes of its vessels, while in man such is the case only in the mucous membrane. Within the lymphatic glands of the mesentery the chyle is once more deprived of its own independent channels; the vasa *inferentia* lose themselves in the porous glandular tissues, from which they join anew to form the vasa *efferentia*. Without further entering here into the minute structure of the lymphatic glands as described by Bruecke, we remark only that he still maintains the opinion expressed in the 'Denkschriften der Wiener Academie' (1850, II. 23), that the lymphatic glands are the seat of the development of the lymph-globules, the germs of which enter into the circulation during the passage of the lymph and chylus through the glands. Amongst the lymphatic glands Beneke enumerates also the glandulæ agminatæ et solitariæ in the various parts of the intestinal tube, the glandular simplices majores (Boehm) in the colon, the tonsils, and some of the glands (the follicular) on the root of the tongue. About the spleen, the thymus, the suprarenal capsules, and the glandular thyroideæ, he refrains from giving an opinion for the present.

IV.—Secretion and Excretion.

1. *Some Experiments on the Secretion of the Urine.* By T. Kierulf, (Zeitschrift für ration. Medicin., von Henle und Pfeüffer.) 1853. H. 3, p. 279.

1. Dr. Kierulf has endeavoured to investigate the influence of considerable dilution of the blood on the quantity and quality of the urine. For this purpose he made the section of the left ureter in dogs (through a wound in the abdominal wall), and collected the urine by means of a glass-tube. On the first dog, the operation was performed between $8^{h}\ 55^{m}$ and $9^{h}\ 40^{m}$ A.M., with only a trifling loss of blood. At $9^{h}\ 45^{m}$, a small venesection (amount not stated); at $11^{h}\ 40^{m}$ injection of 495 (16 ounces) grammes of distilled water (temperature 104·00) through the jugular vein. Five minutes after this, the urine was mixed with a considerable quantity of blood; the quantity of urine much increased. At $11^{h}\ 55^{m}$, second venesection. At $4^{h}\ 9^{m}$ P.M., the urine contained still blood, but in a smaller proportion. At $4^{h}\ 50^{m}$, third venesection. At 5^{h} the urine excreted through the unwounded ureter was likewise bloody. On the following day the urine was normal. Similar experiments on another dog were followed by almost the same phenomena. In order to learn whether the admixture of blood to the urine was caused by the increased pressure of the blood, or by a change in its composition, Kierulf subjected the same dog to another series of experiments; 346 grammes of defibrinated blood (99·5° F.) just taken from another dog, were injected through the vena saphena at $9^{h}\ 25^{m}$ A.M. The urine excreted after this operation remained perfectly clear. At $9^{h}\ 50^{m}$, injection of 495 grammes of distilled water. (99·5° F.) Soon after this, increased quantity of urine, but no change of colour, and scarcely any albumen. At $11^{h}\ 30^{m}$, venesection to the amount of some ounces; at $11^{h}\ 35^{m}$, injection of 490 grammes of water. Soon after this, the colour of the urine deeper, decided reaction of albumen. At $11^{h}\ 50^{m}$ another small venesection, after which the reaction of the urine slightly alkaline, colour rather bloody, quantity of albumen increased. A new injection of 240 grammes at 12^{h}, was followed immediately by a considerable increase of the excretion of urine, with more admixture of blood and albumen. Concerning the composition of the blood detracted by the venesection, Kierulf remarks that, while the proportion of the solid substances appeared diminished, in consequence of the injection, that of the fixed mineral constituents (the ashes of the blood) was increased. Kierulf is inclined to consider the latter circumstance to be the cause of the blood-globules exhibiting an indented appearance under the microscope. The inferences which the author draws from his experiments are as follows:—1. Considerable dilution of the blood produces at first the passage of albumen through the kidneys. He is not inclined to attribute this phenomenon to an augmented pressure of the blood. 2. The normal solid constituents of the urine become decreased, while the proportion of salts appears greater than in the normal state. 3. The quantity of urine excreted within a certain space of time does not correspond to the proportion of water in the blood. Kierulf alludes to the light which may be thrown by similar experiments on the pathology of Bright's disease, but feels himself the necessity of further observations.

V.—Nervous System.

1. *On Certain Functions of the Spinal Chord.* By T. Lockhart Clark, Esq. (Proceedings of the Royal Society.) 1853. P. 297.
2. *On the Insensible Spot of the Retina in the Human Eye.* By Dr. Fick and Du Bois-Reymond. (Müller's Archiv. Heft 3, p. 396.)

The principal results of Mr. Clark's investigations, made on the ox, calf, cat, rat, mouse, and frog, are drawn as follows:—1. That the posterior roots of the spinal nerves consist of three kinds; two of them entering the posterior gray substance at right angles, the third kind with different degrees of obliquity, tending upwards, a small proportion only of the latter taking a longitudinal course, and becoming lost in the posterior white columns 2. That in no instance were any fibres of the anterior roots seen to ascend with the anterior white columns, before they had entered the gray substance. 3. That besides the transverse bundles forming the anterior roots, a continuous system of exceedingly fine transverse fibres issue from the anterior gray substance, and become lost as they proceed towards the surface of the cord. 4. That from the preceding facts; it may be inferred that nearly all, if not the whole of the fibres composing the roots of the spinal nerves, proceed at once to the gray substance of the cord; and that, if any of them ascend directly to the brain, it must be *those only* of the *posterior* roots which run longitudinally in the posterior white columns. 5. That the communication between the sensorium and the spinal nerves is not established by the posterior white columns, but by the antero-lateral columns, especially the lateral. 6. That many of the fibres belonging respectively to the anterior and posterior roots in different regions of the cord, terminate there by forming with each other a series of loops of various sizes and lengths; and that it is not improbable that some of them may reach even as far as the brain. It is not perfectly denied by the author that a portion of the roots may be connected with the vesicles of the cords, but he considers the evidence of any such connexion as very unsatisfactory. 7. The fine longitudinal fibres described by Stilling have not been found by the author. He is inclined to believe that the gray substance of the cord does not transmit impressions to and from the brain. 8 That there is great correspondence in the fibrous arrangement between the gray substance of the cord and the chiasma of the optic nerves The author further remarks that the circumstance of the nerve-roots diverging upwards in the cord and intricately intermingling with each other, may explain why impressions made at one particular spot are communicated to distant parts of the cord, so as to excite simultaneous and sympathetic actions in classes of muscles which otherwise would appear unconnected.

2. Dr Fick and P. du Bois-Reymond differ from Volkmann* in the conception of the influence exercised by the insensible spot of the retina on the perception of the image by the mind They adopt the view that the impressions of light thrown on the insensible spot are not conveyed to the sensorium, while the impressions thrown on the other parts of the retina are perceived by the sensorium. It is well known that Volkmann and other psychologists (Waitz), are of opinion that the soul forms the idea of the dimension of objects by composing the impressions from the various parts of the retina mosaic-like, and that the piece of the object from which the rays are thrown on the insensible spot (which piece may be called "the unseen space"), falls out from the conception The image, therefore, would be constructed smaller than the real object. Volkmann draws from this the inference that a line, the image of which is passing through the unseen spot, must be perceived as much shorter as is just proportionate to that piece of the image which is thrown on the unseen space. In opposition to this theory, the authors maintain the view that the soul possesses in itself a notion of space and dimension, and that it fills up the unseen piece of the object by a kind of *deception* according to certain laws. As one of the principal laws, may be regarded that the quality of the perceptions which the soul fancies to derive from the unseen space depends on the quality of those coming from the immediate neighbourhood. We propose to quote some experiments which the authors adduce in corroboration of their theory. If a black stripe be placed on a white surface in such a manner as to make its image pass through the blind spot and exceed it as well above as below, the stripe is seen *unshortened*, exactly as if its image had been thrown on another part of the retina sensible throughout the whole of its dimension (provided the stripe be not too narrow). If a part of the black stripe is cut out from its central portion and the image of the empty (white) middle space is thrown exactly on the blind spot of

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* Wagner's Handwoerterbuch. Art 'Sehen.'

the retina, while the upper and lower piece remain as in the former experiment (i. e., throwing their image just below and above the blind spot),—the black stripe is seen or fancied *unshortened and entire*, as if the central piece were not cut out. If the image of the stripe is thrown on the retina in such a manner that one *end* of it falls on the blind spot, the *stripe* is perceived by the soul *shorter* than it is in reality, and this shortening is proportionate to that piece of the image which falls on the blind spot, the latter being filled up by the soul with the white colour of the surrounding ground. The authors do not proffer a new opinion about the cause of the insensibility of the blind spot, but on account of the extension and form of the unseen space they are not inclined to adopt the view of its being caused by the arteria centralis retinæ.

VI.—Locomotive Organs.

Experimental Researches applied to Physiology and Pathology. By Dr. Brown-Séquard, of Paris. ('Medic. Examiner,' &c. Philadelphia, 1853. August.)

Dr. **Brown-Sequard** has occupied himself with the study of the nature of the movements of the *contractile tissues* (the muscles of the trunks and limbs, the muscular layer of the digestive canal, the iris, the uterus, the dartos, &c. &c.), which appear to be neither the result of an external excitation nor of an excitation produced by the nervous system. Brown-Séquard applies to these contractions the term spontaneous *contractions*, and considers as one of their causes, if not their only cause, the *carbonic acid* of the blood. This theory is based on the following circumstances:—1. After the section of one of the *facial nerves*, and after the peripheric part has lost its vital property, Brown-Séquard and Dr. Martin-Magron have observed in rabbits *contraction* of the *paralysed* side (so that the mouth deviated to the paralysed side). Whenever the respiration was in some manner prevented or disturbed, they observed the paralytic muscles to tremble, and sometimes even to undergo rhythmical contractions and relaxations. Though in dogs, cats and guinea-pigs, no deviation took place, yet convulsive (sometimes rhythmical) contractions were excited in the paralysed side, whenever the animals were prevented from breathing freely.—2. The spontaneous rhythmical or irregular contractions *in muscles of animal life, after death*.—3. The *deviation of limbs* produced by the contraction of the *paralysed muscles*.—4. The rhythmical movements in the *eye* of the *ink-fish* (Loligo Sepia, L.) after it has been separated from the body.—5. The spontaneous contractions of the *uterus* at a time when the spinal cord has entirely lost, not only its reflex power, but also the power of acting on muscles when directly excited by galvanism, warmth, or mechanical stimuli.—6. The spontaneous rhythmical movements in the *crop* and *œsophagus* of *pigeons* and other birds.—7. The spontaneous movements in the *limbs* of persons who have died of *cholera*.—8. The spontaneous contractions of the *bowels*, the *bladder*, the *iris*, and other *contractile parts* of the body at the time of death, and soon after death.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

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I.—THE ACUTE SPECIFIC DISEASES.

1. *The Indian Plague and the Black Death.* By Dr. HIRSCH. (Virchow's Archiv fur path. Anat, Band v. Heft 4. § 508)
2. *Abdominal Typhus and Cholera Typhoid.* By Professor VIRCHOW. (Verhandl. der Gesell, in Würzburg Band iv. Heft 1. § 77.)
3. *The Urine in Typhus and Typhoid Fever.* By Dr. G. W. EDWARDS. (Edin. Monthly Journal. Sept)
4. *The Irish Fever of* 1817. By Dr. PUREFOY. (Dublin Quarterly Journal. Nov)
5. *The Blood in Cholera.* By Dr. ROBERTSON. (Edin. Monthly Journal. Sept.)

1. UNDER the title of 'The Indian Pest and the Black Death,' Dr HIRSCH has published an historico-pathological sketch of the disease termed in India the Pali Plague, and of the affections noted in times gone by which are more or less closely allied to it. In his account of the 'Pali Plague,' the author refers especially to the following documents Papers by Messrs Maclean, Irvine, Glen, and Panton, in the 'Calcutta Quarterly Journal of Medical and Physical Science,' for 1837; papers by Whyte, M'Adam, Gilder, and Forbes, in the 'Bombay Transactions' for 1838 and 1839, Ranken's 'Report of the Pali Plague,' and Webb's 'Pathologica Indica' The Pali Plague (so named from its commencement, in 1836, at Taiwali, near Pali in Marwar) was noted first in 1815, in Kutch and Guzerrat; it prevailed epidemically till 1821 in the adjoining countries, and then ceased It recommenced in 1836 in Marwar, and extended itself for three or four years, when it ceased to be heard of. In 1850 a new outbreak occurred in Gurhwal and Kamoon ('London Medical Gazette,' vol. xi. p. 349), but no further intelligence of this has reached Europe. The symptoms of this disease closely resembled those of the Levant or Bubo plague: shivering, heat, pains in head, back, and limbs, great loss of strength, quickened pulse, hot and dry skin, intolerance of light, redness of the conjunctiva and of the face generally; then followed vomiting of bilious or coffeeground-like matters; the abdomen became hard, distended, and constipated, except towards the end of the case, when there was sometimes bloody diarrhœa; there was intense thirst; the urine was scanty and high-coloured, sopor and delirium were usual symptoms. On the second or third day appeared the distinctive marks of the disease—viz, either buboes, or a peculiar lung affection, or both. The lung affection consisted in severe sternal pain, dyspnœa, cough, with expectoration of either almost pure blood or of mucus deeply tinged with blood. The buboes appeared chiefly in the left groin, seldom in the axillæ. In fatal cases all the symptoms increased in severity, and death occurred on the third day. If the patient lived beyond the fourth day recovery was common; in this case the buboes either rapidly suppurated or became hard, and then gradually retrogressed. The occasional separation of the glandular and lung affection led to two chief varieties being distinguished, of which the lung affection was by far the most fatal. The total mortality was as high as 75·80 per cent. of those attacked.

The author believes (although there are no records of accurate physical investigations of the lung, or of post-mortem examinations) that the lung disease consists not in an exudative pneumonia, but in hyperæmia of the bronchial mucous membrane and hypostasis of the lung parenchyma. In this disease the author sees "a [illegible] bubo plague." He denies its spread by contagion, and refers to Allan

Webb to show that it is endemic in some districts (Gurhwal), and occasionally becomes epidemic, and therefore that it was not derived from the Levant, as some of the Indian observers suggested. He then proceeds to trace its connexion with former pestilences. He refers to the latest accounts of the Egyptian epidemics (Robertson 'Edin. Med. and Surg. Jour.,' 1844; Pruner, 'Krankh. des Orients;' Delory, 'Rapport sur la Peste,' in the 'Rapport à l'Acad. Royale de Méd.,' by M. Prus), to show that in this disease there is no account of any lung affection similar to that of the Pali Plague. He then goes back to the earlier Egyptian epidemics (as recorded by Clot Bey, Bulard, Aubert, Iken, &c.), and to the reports of the Russian physicians on the plague at Odessa in 1827, and then to still earlier accounts (Orræus, Russell, &c.), and can find in none of them any description of pulmonary disease which accords with that given by the Indian observers. Only in one or two German writers of the beginning of the last century, and in one passage in Diemerbroeck ('De Peste,' libri iv. Arenaii, 1646) he finds brief references to cough and bloody sputa as a very infrequent occurrence.

When, however, the author passes further back to the account of the "Black Death" of the fourteenth century, he finds the description of this disease accord with that of the Indian Pali Plague, and he agrees with Hæser, that the "Black Death" was a modified bubo plague, with a peculiar, and hitherto unknown, pulmonary complication. He therefore quotes with approbation the conclusion of Allan Webb, that the "Pali Plague exactly resembles the Black Death."

Finally, the author demands whether, in the thirteenth century, the Black Death did not spread from India, just as in these days we see the Asiatic cholera taking its rise in the same districts, and passing apparently somewhat in the same way over the face of the earth.

2. Professor Virchow relates, at great length, a case of typhoid fever in which some of the symptoms during life, and some of the post-mortem appearances, had a choleraic character. The patient (a woman aged thirty-three) was attacked suddenly with shivering and headache, followed by persistent profuse serous diarrhœa and repeated vomiting. On admission on the fourth day the temperature of the skin was 99·5° Fah.; the pulse 95, small; there was no tenderness of the abdomen; the diarrhœa and vomiting continued. On the four following days the same symptoms; the temperature varied between 99° and 100°; on the following day it rose to 101°. On the tenth day there was some collapse; the diarrhœa and vomiting were less; on the 11th "little rosy red spots" appeared on the abdomen and breast. On the following day there was rather unexpected and sudden death. The condition of the urine is only once noted, two days before death; it was then non-albuminous. On post-mortem examination, in addition to other things, there was unequivocal typhoid exudation into many, and ulceration of one, of the glandulæ agminatæ, and infiltration of the mesenteric glands.

Virchow remarks that this was undoubtedly a case of typhoid fever, but he observes that in the section a series of appearances presented themselves which are found in cholera; there was a cyanotic condition of many peripheral parts; a rosy hyperæmia of the small intestines (which Pirogoff and Virchow consider so characteristic of cholera), and an extensive desquamation of the epithelium, of the intestines, stomach, and gall-bladder; there was also the parenchymatous change in the kidney which Virchow has formerly described as frequent in cholera-typhoid.

During life also the case was noted for the slightness of the febrile and nervous symptoms, and for the extraordinary severity of the serous catarrh of the gastro-intestinal mucous membrane. Altogether, Virchow believes that the resemblances to cholera were sufficiently striking, yet in the place where this case occurred (Würzburg) no cholera has ever yet been seen, and he contents himself, therefore, with directing the attention of pathologists to unusual cases of this kind, with a view to determine whether a combination of processes, so different as the febrile and choleraic conditions appear to be, may not be possible.

[We cannot quite understand the importance which the author attaches to this case. The choleraic symptoms scarcely appear sufficiently marked; the vomiting and purging, *per se,* and the so-called choleraic after-death appearances are scarcely sufficient, we think, to warrant the inference drawn from them.]

3. Dr. G. W. Edwards has published an interesting paper on the urine in typhus and typhoid fevers. In the former disease the urine "is generally pale, is of rather low specific gravity, and, in a majority of cases, contains albumen at an early period of the disease." In typhoid fever the urine is high-coloured, is of high specific gravity, and does not contain albumen, except occasionally towards the end of the case, but gives, with a little acid, a dense non-albuminous precipitate soluble in excess of acid, and consisting of urates.

[It would have been desirable to have known the *quantity* of urine in these cases of typhus and typhoid; it is, we believe, often copious in typhus, and scanty during the first fourteen to eighteen days in typhoid; but after that date the quantity increases, the colour becomes paler, and the acidity diminishes. The precipitate of urates caused by a drop of acetic or hydrochloric acid is common in many diseases.]

4. The Irish Fever of 1847 is stated, by Dr. Purefoy, to have been of remittent character, and to be a "new, or peculiar and specific form." The disease is said to have suddenly commenced with chills, headache, pains in the limbs, nausea, and hot skin. After a few days marked remission of all the symptoms followed, so that the patient left his bed, when a fresh accession of febrile symptoms, or, "as it was generally termed, a relapse," occurred. These exacerbations occurred regularly every 5th, 7th, 11th, or 15th day in some cases, but in others 5, 15, or 21 days elapsed between the attacks. When the disease had lasted 3 or 4 weeks, the exacerbations were severe, and were attended with vomiting and profuse diarrhœa. Maculæ were frequent in the beginning of the epidemic. Among a registry of 260 fever patients, there were 256 of this fever, and only 6 cases of true typhus.

The writer states that, since 1847, this form of remittent fever has continued to prevail in Ireland. "The marked features in this novel form of fever may thus be summed up. a previous state of slight general indisposition of days' or weeks' continuance, followed by chilliness or rigors, and the usual symptoms of mild fever; deceptive symptoms of crisis occurring at an uncertain period of the disease, succeeded by apparent convalescence,—this amendment being of short duration,—when the disease re-appears in a more aggravated form; these *crises and relapses* (improperly so called) being the *true and peculiar characteristics* of the disease now described, which may be justly styled remittent in its nature, symptoms, and progress."

In the treatment blood-letting is recommended if the patient be not already much reduced; purgatives are to be avoided, since the intestinal mucous membrane is apt to suffer Unduly prolonged diaphoretics are also hurtful; alkalies, prussic acid, and vegetable bitters, are useful, and quinine, though it does not prevent the paroxysms, delays their return. Change of air, as soon as the patient can bear it, is an important remedial measure.

5. Dr. William Robertson gives us the results of the chemical examination of the blood in thirty-five cases of cholera, occurring in 1848-9. The process adopted was that of Christison, as used by Andral. The results are given under four heads—

		Early Stage, 7 cases.	Incipient Collapse, 6 cases.	Complete Collapse, 14 cases	Reaction, 8 cases.
Fibrine		2·7	3·2	3·2	3·7
Serous solids	Organic	82·2	93·4	102·4	78·2
	Inorganic	7·8	6·9	6·9	6·6
Globules		103·4	129·9	129·9	122·6
Total solids		196·1	233·4	242·4	211·1
Water		803·9	766·6	757·6	788·9
Specific gravity of blood		1053·1	1059·5	1060·8	1055·8
,, ,, serum		1029·4	1033·7	1030·9	1030·8

In other cases, not recorded, the blood was examined for urea, which, in two cases, amounted to 1·6 and ·73 per 1000.

Dr. Robertson observes that the table warrants the conclusions—that anæmic persons are more predisposed to cholera; that the changes which affect the blood are loss of water, and some loss of salts; that in the period of reaction the serum is rapidly diluted, and that the salts are therefore still more (relatively) diluted. He does not, however, know how to explain the rapid diminution of the albumen which occurs in reaction, with the increase in the fibrine, and the almost stationary condition of the red corpuscles.

II.—THE NON-SPECIFIC GENERAL DISEASES.

1. *On Tuberculosis in Egypt.* By PROFESSOR GRIESINGER. (Vierordt's Archiv für Phys. Heilkunde, 1853. Heft 3. pp. 519.)

1. IN 363 dissections at Cairo, by Professor GRIESINGER, there was tubercle in 62 (17 per cent.), but as in 12 it was very trifling and obsolete, it should be said that there was recent tubercle in 50 (13·8 per cent.). (In Stuttgardt and Prague the proportions are, according to Cless and Dittrich, whose observations are referred to for comparative data, 36—37 per cent. in both places.) It was less common in old persons; its greatest frequency was between the ages of 15 and 20; but in general terms it may be said to have been nearly the same between 7 and 40 years. Among the 363 dissections in the hospital were 333 Fellahs and 10 Negroes; the proportion of tubercle was only 11·11 per cent. among the former, and no less than 50 per cent. among the latter. Dr. Griesinger remarks that the disposition of Negroes to tubercle, so common in cold climates, begins already in Egypt. With respect to the implication of particular organs—the lungs were unaffected in one case in which there was tuberculous meningitis; in all other cases they suffered. In 33 cases the disease was confined to the lungs and its appurtenances (pleura and bronchial glands); in 10 cases the lung disease was about equally advanced with disease of other organs; in 6 cases the disease was very trifling in the lungs, but was advanced elsewhere.

The amount of disease in the lung appeared less than in phthisical cases in Europe; the lower lobes alone were attacked in four cases, the extreme apices of the lungs appeared to be spared often, and the tubercle was found about the height of the second or third rib.

In 4 of these 50 tuberculous cases there was pericarditis (not apparently with tuberculous deposit at that point). The peritoneum was tuberculous in 14 cases (28 per cent., whereas in Cless' cases it was affected only in 13 per cent., and in Dittrich's in 7 per cent.). The small intestines were affected 23 times = 46 per cent. (in Cless' cases 54 per cent.); the large intestines were affected in 6 cases = 12 per cent. (in Cless' cases 24 per cent.). The intestines were thus altogether affected in 50 per cent., while Cless' numbers are 78 per cent. and Dittrich's 72. The mesenteric glands were affected in 22 cases = 44 per cent. (in Cless' cases 25 per cent.); the liver was tuberculous in 9 cases = 18 per cent., (in Cless' cases only 1 per cent.); the spleen was affected in 23 cases (46 per cent.), and between the ages of 7 and 30 this organ was affected in no less than 87 per cent.; in Europe the frequency of spleen-tubercle is much below this; the kidneys were affected 12 times (= [illegible] per cent.); this number is also much higher than in Europe;

In 9 cases there was tuberculous meningitis; [illegible] following is the order in which the organs were attacked: lungs, bronchial glands, spleen, small intestines, peritoneum, pleura, kidneys, mesenteric glands, liver, large intestines, pia mater, brain.

Dr. Griesinger then remarks that tuberculosis generally, and phthisis pulmonalis in particular, are far less common in Egypt than in Mid-Europe; the causes of this are, perhaps, the mild climate, the mode of occupation, which is never hardly sedentary, and the infrequency of bronchitis and inflammatory affections of the lungs. The investigations show also the relative infrequency of tuberculosis in children; while, on the other hand, the extremely frequent implication of the mesenteric glands, peritoneum, liver, spleen, and kidneys, makes the tuberculosis of adults in Egypt approach, as far as organs are concerned, the tuberculosis of children in Mid-Europe.

'The important question whether Egypt (Cairo) is a good residence for tuberculous Europeans, is answered by Dr. Griesinger in the affirmative, and cases are referred to in which the disease was decidedly arrested.' Nevertheless, the disease should be in an early stage, and without bowel implication, as dysentery is very apt to ally itself to it. The patients should arrive in October in Alexandria; should go to Cairo in November, and there remain, or go to Upper Egypt or Nubia. In March, or at the beginning of April, they should leave Egypt, and go to Syria.

III.—THE DISEASES OF THE THORACIC ORGANS.

1. *The Examination of the Size and Situation of the Organs of the Chest.* By DR. CONRADI. (Archiv des Vereins. Band i. Heft 1.)
2. *On Laryngeal Ulceration.* By DR. RHEINER. (Virchow's Archiv fur Path. Anat. Band. v. Heft 6. pp. 534.)
3. *On Interlobular Pneumonia.* By DR. WEBER. (Virchow's Archiv fur Path. Anat. Band. vi. Heft 1.)
4. *On Diaphragmatic Pleurisy.* By M. GUÉNEAU DE MUSSY (Archives Générales.)
5. *On Thoracentesis.* By DR. SCHNEPF. (Archives Générales. Oct.)

1. THE following extract contains an abridged statement of Dr. CONRADI's paper on the method of examination practised in the "Medical Clinik," at Giessen, under the direction of Professor J. Vogel.

The position usually preferred, if the person can be out of bed, is the upright, with the arms close to the sides; if in bed, the patient lies flat on the back, (the pillows, &c., having been removed,) for the examination of the front, and then sits up for that of the back. The time of meals, the quantity of food or fluid taken, are noted.

Examination of the thorax. At first general inspection of the formation of the chest, with particular attention paid to the so-called "angulus Ludovici," (prominence produced by the union between the manubrium and body of the sternum.) Then exploration of the *diameters* of the chest. Three *transverse* and two *antero-posterior* (*gerade*) diameters are adopted. The instrument of which use is made for measuring them consists of a beam divided into centimeters,* of two arms sliding thereon, and a centrepiece. For measuring the transverse diameters, the centrepiece is applied to the median line of the thorax, (from the incisura semilunaris sterni to the processus xiphoideus,) while both arms of the instrument are applied in equal height to the sides of the body. The antero-posterior diameters are found by merely applying both arms of the instrument. The *first transverse diameter* goes from one fossa axillaris to the other. The centrepiece is applied to the angulus Ludovici, the arms of the instrument as nearly as possible to the ribs, which is assisted by varying the degree of abduction of the arms from the thorax, (from 50 to 60°). The *second transverse diameter* lies in the height of the ninth rib. The centrepiece is placed on the basis of the processus xiphoideus, the arms on each side over the ninth rib. The *third transverse diameter* is determined by applying both arms of the instrument to the eleventh rib, and the centrepiece to the median line of the abdomen (line from the point of the processus xiphoideus to the symphysis pubis.) The *first antero-posterior diameter* extends from the angulus Ludovici to the processus spinosus of the third and fourth dorsal vertebræ; the *second* from the basis of the processus xiphoideus to the processus spinosus lying nearest to the line drawn from the lower angulus of the one scapula to that of the other. The width of the shoulders is determined by applying the centrepiece to the angulus Ludovici, and the arms to the deltoid muscles, the degree of development of which is always described. To aid the exploration and description of the size and situation of the internal organs, the following four lines are adopted:—1. The *linea mediana* (see above). 2. The *linea mammalis:* from the union between the anterior and middle third of the clavicula downwards through the nipple. 3. The *linea axillaris:* from the middle of the axilla to the anterior end of the eleventh rib. 4. The *linea cristo-dorsalis:* from the middle of the spina scapulæ downwards parallel to the internal margin of the scapula and the external prominent ridge of the muscul. iliocost. (sacro lumbal.)

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* 1 Centimeter = 0·3937 inch of English measure.

As to the exploration for the size of the single organs, the following remarks are made. 1. For *the right lung.* Percuss from above downwards in the just mentioned lines, and mark exactly where you meet in every line, at first the less sonorous and then the perfectly dull sound. Conradi found the average distance between the point where the sound became less sonorous and that where it became perfectly dull, to be about 3 centimeters (=1⅕ inches) in healthy male individuals between 20 and 30 years. In the linea mammalis, he marked in general the beginning at the 5th intercostal space; in the linea axillaris, at the 7th rib or 7th intercostal space; in the linea cristo-dorsalis, at the 9th intercostal space. During deep inspirations, the lower margin of the lung descends about 1½ to 2 centimeters below the line where it is marked during normal inspiration; it is raised on the other hand about 1 to 2 centimeters over its level by forced expiration. The examination must therefore be always made during quiet respiration. The percussion on the posterior wall ought to be more powerful than on the anterior; it must be slight for the exploration of the lower border of the lung, strong for that of the superior border of the liver.

2. For the *examination of the left lung* percuss in the linea mammalis, axillaris, and cristo-dorsalis downwards until the dulness from the heart and spleen is reached. The line which unites the point of the former with that in the linea axillaris, pointing out the beginning of the dulness from the spleen, is considered to indicate the situation of the lower border of the left lung. Conradi in general met with the beginning of the heart's dulness at about the 4th intercostal space, from whence he could, by slight percussion, trace the internal margin of the lung covering the heart directed in a transverse line outwards and downwards to the left until to the 6th rib. In the linea axillaris he mostly found over the 8th rib or intercostal space, the inferior margin of the lung meeting the superior of the spleen. In the linea cristo-dorsalis, he constantly met with the dulness from the spleen on the 10th intercostal space.

3. For *the internal margin of the lungs and the heart,* percuss in the median line from above downwards, to ascertain the beginning of the heart's dulness; starting from thence, explore by light percussion the internal margin of the right lung, which in most cases may be traced from the 4th left intercostal space downwards on the left side of the sternum as far as the union between the sternum and xiphoid process, where it passes over to the right side, and is farther explored as described above. In the examination for the heart, a distinction is made between the space of complete dulness (*Herzleerheit*), produced by the part of the heart situated immediately below the wall of the thorax, and that of incomplete dulness (*Herzdaempfung*) by superposition of the borders of the lungs over the heart. The former is detected by light, the latter by powerful percussion. It is recommended to percuss in various directions, from the circumference of the thorax towards the heart; on the right side, principally in a line parallel to the superior margin of the liver, and in another direction from the point of the 3rd intercostal space, situated in the linea mammalis, inwards and downwards; on the left side, in the same line, and in another from the 5th intercostal space towards the sternum. Conradi found, on the right side, on percussing in the inferior line, the dulness from the heart reaching beyond the margin of the sternum; in the superior he met with

it just at the margin, or between it and the median line; in the upper line on the left, over the 4th intercostal space; in the lower over the 5th, slightly outwards of the impulse. The highest point of the dulness was in general met with in the height of the 3rd intercostal space on the left margin of the sternum. By uniting these 5 points through lines, a conical figure is produced, showing the circumference of the dulness from the heart. Within this figure we may trace another triangular one, indicating the space where sound of percussion is completely toneless; the lateral borders of this figure are formed by the internal margins of the lungs, the basis by a line extending from the point where, on the right side, the dulness from the heart and liver meet with the extreme point of the heart's dulness on the left. The superior angle (the point of diverging of the internal border of both lungs) is found in general at the 4th left intercostal space; the right lower angle in the middle of the processus xiphoideus; the left in the 5th intercostal space, slightly inwards of the linea mammalis. The basis, therefore, of both figures is given by the same line.

2. Dr. **Rheiner** has written a long and able paper on ulcerations of the larynx. He briefly notices, first, the alterations which occur after death in the diseased laryngeal membrane; the redness disappears, especially at the points where the elastic elements of the mucous membrane are abundant; swelling, also, disappears, if it be occasioned by fluid exudation; simple softening of the mucous membrane is a post-mortem appearance, dependent either on decomposition or maceration. He then considers ulcerations; 1st, *as regards their anatomical origin.* Disease may commence externally in the structures of the neck, from abscess or tumour, and penetrate inwards; more frequently it commences in *perichondritis laryngea,* and passes inwards; finally, the ulceration may begin on the internal surface of the mucous membrane and pass outwards. 2nd. *As regards their seat.* In phthisis the posterior commissure of the vocal chords, and the bases of the arytenoid cartilages, especially suffer; in syphilis, and in follicular pharyngeal disease, the parts above the chordæ vocales are most concerned; perichondritis laryngea implicates especially the cricoid, according to most authors, but according to Dr. Rheiner, the arytenoid. 3rd. *As regards their etiology.* Ulcerations are now and then of traumatic origin; they occur in syphilis, sometimes in typhoid fever, very frequently in lung disease, and sometimes in pharyngeal disease. Into the connexion between lung and laryngeal diseases the author enters rather fully, and discusses the possible origin of laryngeal ulceration from disease of the nerves, from participation in the specific lung-disease (pneumo-tuberculosis), from "infection" (as some have supposed), from irritation from the sputa, &c., and decides that at present a plausible explanation of the connexion between laryngeal and lung disease cannot be given.

The author then proceeds to consider laryngeal ulceration from an anatomical point of view, and makes the following varieties:

(a.)—*The simple catarrhal ulceration,* which is preceded by catarrhal inflammation. This includes many specific ulcerations, as those of syphilis and of phthisis. The author describes the *macroscopic* characters of the ulcer in the ordinary way; then passing to the *microscopic,* he observes that the first step of the process is the dislodgment of the epithelium (as occurs, indeed, in even simple inflammation without ulceration); the condition of the "homogeneous layer" immediately beneath the epithelium cannot be well made out; the tissues of the mucous membrane become crowded with little cellular elements (as occurs also in old catarrh without ulceration) to such an extent as to call to mind the iliac swellings in typhoid fever; these cells are heaped together in masses, and arise from "proliferation" of the pre-existing normal elements; when in great amount they may press on the vessels and impede the circulation of the part; they may then form pale prominences, visible to the naked eye, which may be called tubercle; the tissues enclosed between these cells, and often the cells, are then set free, and are thrown off and form part of the sputa; in this way an ulcer is formed and extends itself. In a healthy larynx no papillæ are found on the mucous membrane, but papillary or villous growths are found on the borders of the ulcers, and are pierced with vessels; sometimes they become so large as to be true vegetations. At a greater distance from the ulcer, the mucous membrane is tumefied from serous effusion; and the corpuscles of the uniting tissue are larger. The perichondrium does not long escape alteration; traces of increased cell-formation appear here also, but on account of the toughness of the membrane, it does not readily ulcerate. The alteration in the cartilages from neighbouring ulceration is difficult to determine, as these structures are so prone to alteration from age: still it is known that an habitual catarrh of the larynx without ulceration hastens the ossification (calcification) of the cartilages; ulceration appears to have the same effect, that part, of course, being chiefly or wholly affected which is nearest to the ulcer; for ossification to take place, however, the ulcer must be of some standing. The author then discusses the question of ulceration of the cartilages, and sums up thus:—It may occur in two ways—*first*, in perichondritis; then if the cartilages have been previously ossified, as in old persons, necrosis or caries may ensue, according to the amount of deprivation of blood-supply; *secondly*, from ulceration of the mucous membrane passing outwards; and here, if the ulcer be chronic, and not in a very young person, ossification occurs; especially at the adjacent borders of the cricoid and thyroid, if the ulcer happens to be situated here. The necrosis or caries may come on as in perichondritis, or if not, the cartilage, from loss of nutrition, becomes yellow or brownish, and softened to a jelly-like consistence. If, however, the cartilage continues to be sufficiently connected with the vascular parts to preserve its nutrition, then ensue the changes described by Redfern and Virchow. The cells exhibit a greater number of fat globules, and are in fact filled by these; the nuclei are divided, and give rise to a number of shining corpuscles in the cells or in the intercellular tissue, when the cells are broken down.

The condition of the bloodvessels in ulceration is determined with difficulty; they [illegible] compressed and obliterated by the formation of cells, or disappear before the ulceration; the nerves, on the other hand, are destroyed with difficulty, and are sometimes seen on the ulcerated surface as white fibrillæ, which on microscopical examination are found to be unaltered nerve-fibres. The follicles suffer in catarrhal inflammation no special alteration. A variety of the catarrhal ulcer is the superficial aphthous erosion of the larynx and trachea.

(b.)—*The Follicular Ulcer.*—In catarrhal inflammation the glands suffer only in common with the intermediate membrane; in this disease they suffer very differently; in their interior an important cell-growth occurs and distends them to the utmost; the delicate uniting tissue between them becomes thinned; the follicular walls come in contact, then burst, and the union of several follicles forms a cavity which is surrounded by areolar tissue, and forms a prominence above the level of the mucous membrane; at its apex is a yellowish white point, from which fluid can be pressed; at last the walls at the prominent part burst, the contents are discharged, and an ulcer is formed.

(c.)—*The Diptheritic Ulceration.*—The author relates in illustration one case of croupo-diptheritic inflammation of the pharynx, larynx, and trachea, in which in the pharynx and the highest part of the larynx there was also loss of substance of the mucous membrane.

(d.)—*The Syphilitic Ulcer.*—Many syphilitic ulcers are of catarrhal origin, but in these cases there is also peculiar disposition to the formation of vegetations.

3. Dr. **Weber** describes under the name "interlobular pneumonia," the morbid anatomy of the disease in cattle known by the name of the "lung pestilence," and states that he has observed a similar affection in the lungs of men. In the lungs of cattle the interlobular uniting tissue is much more developed than in men; like the pleura, it is fed by branches of the bronchial artery. On account of this latter circumstance, the pleura is always attacked at the same time as the interlobular uniting tissue. This is the case also in interlobular pneumonia in men. On accurate examination of a lung thus affected, besides the pleuritic effusion, the surface of the lung

showed a dark colour, diversified, however, by numerous white-yellow streaks, three or four lines broad, which surrounded the lobules. The lung was heavy; when cut there was considerable resistance; the section showed hundreds of little dark red islands formed of congested lobules, separated by white-yellow streaks of altered interlobular tissue, as on the surface. On further examination, the mucous membrane of the greater and smaller bronchial tubes showed no trace of croupous exudation; it was normal in the larger, and in the smaller bronchi had only a slight catarrhal injection. The vessels of the air-cells were greatly engorged, but the cells themselves were filled with fluid (œdema pulmonum), not with firm exudation, except in some very few instances. In some cases there were greater or less sized extravasations of blood. The exudation into the interlobular substance was not microscopically distinguishable from other inflammatory exudations; it was frequently more or less organized into new uniting tissue; especially at the outer portion of the streaks: the inner portion was less advanced, and this at a future period often softened.

The coats of the larger branches of the bronchial arteries were thickened and surrounded by organized exudation; often the calibre was blocked-up by fibrinous coagula. The smaller branches, even to the capillaries, were lying in the exudation in the interlobular tissue, and were often much dilated and blocked-up with coagula. This blocking-up of the vessels is considered by the author to be secondary.

An after-stage of this condition was given by softening of the unorganized mass which lay in the centre of the more developed exudation; purulent collections were found, in which sometimes large masses of lung substance floated loose, which in some cases, though not in all, were gangrenous.

In some cases these pieces of detached lung became surrounded by a capsule, so that it might have seemed to be a tuberculous cavern, only the examination of the encapsuled piece of lung showed the lobular structure.

In the commencement of this disease, the interlobular uniting tissue was hyperæmic, somewhat swollen, and yellowish in colour; at the same time the pleura underwent a similar change; then an amorphous thin exudation occurred in both places. At this time the capillaries of the air-cells often showed little hæmorrhages, which gave the section a dark pointed appearance. Then occurred acute œdema of the air-cells, and no air was able to penetrate into them.

In human lungs a similar process, though to a much less extent, has been traced by the author, and in one case he found a separated piece of lung enclosed in a fibrinous capsule, as is so frequent in cattle. In the pleuritis of children the interlobular tissues often also suffers. The author refers to a future paper he intends to publish on the interlobular pneumonia of men.

4. M. **Guneau de Mussy** has written a long memoir on diaphragmatic pleurisy. He observes that the ancients considered delirium to be an urgent symptom of inflammation of the diaphragm, whence arose one of the terms applied to the structure (φρενες, diaphragm). This opinion was generally accepted until its incorrectness was vigorously proclaimed by J. P. Frank; but even at the present time, "periphrenitis" is often used to denote inflammation of the diaphragm, and of the pleura covering it.

Laennec does not seem to have considered the diagnosis of this affection possible, except when the ordinary signs of pleurisy succeeded, such as dulness on percussion and abolition of the vesicular murmur, and sometimes ægophony.

Diaphragmatic pleurisy presents several varieties; sometimes the inflammation is concentrated between the base of the lung and the diaphragm; sometimes, having commenced here, it spreads to the costo-pulmonary pleura. In a third variety it begins in the costo-pulmonary pleura, and thence spreads to the diaphragmatic [illegible]

The chief object which the author has in view is the diagnosis. This is made by attending to the following symptoms which accompany the febrile condition:

(a.)—The pain is severe and radiates from one or other hypochondria, along the costal edge to the epigastrium; it is increased by pressing from the abdomen upwards, and there is also a spot about two fingers' breadth from the middle line at the height of the 10th rib, which is peculiarly tender on pressure; close to the spine in the last intercostal space is another tender point.

(b.)—Pressure between the two insertions of the sterno-mastoid gives pain from the pressure on the phrenic.

(c.)—There are frequently spontaneous supra-clavicular and scapular pains. There are sometimes twitches of the face.

(d.)—The attitude is peculiar; the patient is usually in a sitting position.

(e.)—The dyspnœa is urgent; the respiration chiefly, but not entirely costal.

(f.)—The cough is dry, or with a little mucus; there are no physical signs except diminution of vesicular murmur at the base, and perhaps sub-crepitation from some pulmonary congestion.

Some of these symptoms occur in other cases; as, for instance, there is sometimes pain in the neck and over the course of the phrenic in pericarditis, but if these symptoms are present, and positive signs of other diseases are absent, the diagnosis of diaphragmatic pleurisy may be made

The author (contrary to Laennec) speaks doubtingly of the prognosis. On the subject of treatment, nothing new is communicated.

5. Dr. Schœpf relates 3 cases of acute, and 1 of chronic pleurisy, treated by thoracentesis, and discusses at length the advantages and supposed dangers of the operation. While he believes it to be useful in cases of simple pleuritic effusion, he holds it to be hurtful when asphyxia is imminent and the forces are prostrate, and therefore he dissents from the views of those who employ it merely *in extremis*. He also declares that the entrance of a *small* quantity of air into the pleural cavity is *useful*, as aiding the outflow of the fluid. In the first case the operation was performed on or about the 13th day of an acute and rapidly increasing pleurisy, and 29 ounces of fluid were removed. The symptoms were at once relieved, and the recovery was complete in 18 days afterwards. In the second case the operation was performed on the 12th day of an acute pleurisy, and 35 ounces of fluid were removed; the symptoms at once improved, and the patient left the hospital 23 days afterwards. In the third case, chronic, and in a tuberculous subject, 32 ounces of fluid were removed with temporary benefit, but the effusion returned; after, from the influence of medicines, a partial absorption occurred. In the 4th case, one of acute pleurisy, thoracentesis was performed on the 21st day, and 43 ounces were removed. The benefit was very marked, and 9 days afterwards the patient left the hospital.

In all these cases the after-treatment seems to have been very simple; after the operation a troublesome cough came on in some of the cases, which is attributed to the penetration of air into the pulmonary vesicles which had been previously compressed by the effusion.

IV.—THE DISEASES OF THE DIGESTIVE ORGANS.

1. *The Examination for the Size and Position of the Liver and Spleen.* By Dr. Conradi. (Archiv des Vereins. Bd. i. Heft 1.)
2. *Black Fur on the Tongue.* By Dr. Eulenberg. (Vierordt's Archiv für Phys-Heilkunde. 1853. Heft 3. S. 490.)
3. *The Pathology of the Pancreas.* By Dr. Eisenmann. (Prag. Vierteljahrsch. 1853. Bd. iv. S. 73.)

1. In the paper referred to in a previous section, Conradi refers to the *exploration of the circumference of the liver*. The upper border is considered to lie where the sound elicited by percussing from above downwards in the lines formerly mentioned, commences to be less sonorous. To ascertain the diameters of the liver, the breadth is measured in the linea axillaris, mammalis and mediana. In describing the breadth of the left lobe to the left of the linea mediana, the distance from that line must be always noted. It must also be stated in each case how far to the left

of the linea mediana the dulness may be traced. As the result of the examination of 50 healthy male individuals, between 18 and 25 years of age, Conradi states—1. That the border of the liver is met with in general at the 7th rib, or intercostal space in the linea axillaris, at the 5th rib or intercostal space in the linea mammalis.—2. That the situation of the lower border differs widely within the limits of health. In the linea axillaris the lowest was found 46^{m} in the linea mamm. 76^{m}. below the arcus costalis. The dulness from the border of the left lobe coincides almost with a line from the umbilicus to the left angle of the dulness from the heart. — 3. The diameters do not increase in proportion to the height of the individuals.—4. The lower lobe of the right lung covers a rather larger portion of the liver in taller than in shorter individuals.

With respect to the *spleen,* Conradi tried particularly to explore the situation of the anterior end of its breadth in the linea axillaris and cristo-dorsalis. The situation of the anterior end is described by noting the distance between the linea axillaris and another line, the *linea costo articularis* (adopted by Hamernjk, passing from the point of the 11th (floating) rib to the articulatio sterno-clavicularis.) To explore the longitudinal diameter, the use of the above-described instrument is advised, in order to avoid the variations produced by the difference in the convexity of the ribs when measured with tape, &c. After having explained the difficulties connected with an accurate exploration of the size of the spleen, Conradi states as his experience—1. That the anterior end mostly reaches to the linea costo articularis, but that considerable enlargement may exist without its passing this line. In the linea axillaris the superior border is generally met with in the 8th intercostal space or 9th rib, the lower border in the 10th intercostal space or 11th rib. 3. In the linea cristo-dorsalis the superior end is found in most cases at the 9th rib.—4. The longitudinal diameter, when taken by the instrument, measures about 13½ centimeters (called the *straight* diameter), when taken by a piece of tape over the convexity of the ribs 19½ centimeters (called *convex* diameter).

2. Dr. EULENBERG records a case of a boy aged 2 years, whose tongue from apex to base was thickly coated with a perfectly black layer. At first attributed to the use of some article of diet, it was soon seen to arise from a black formation on the tongue itself. When removed, the tongue commenced to become again coated in the centre, and then towards the tip, and then backwards to the very root. All the papillæ were very large, and the papillæ vallatæ in particular remained black even when the rest of the tongue was clean. This condition of tongue continued for three months. At first the child had also a little diarrhœa; this ceased, and the general health was unimpaired. Under the microscope there were numerous very much thickened brown-coloured epithelial cells, and between and especially on their borders were numerous pigment-corpuscles, which were sometimes arranged mosaic-like, and were enclosed by no cell or membrane. After the case had been observed for some time, it was treated and completely cured with chlorine water. Dr. Eulenberg remarks that this is a case of formation of pigment without any appreciable impairment of the general health, and reminds us of the cases in which black pigment appears in the sputa of simple catarrh.

3. Dr. EISENMANN assembles in an instructive paper all the recorded cases of fatty discharges connected with pancreatic disease, in order to see whether the opinions of Bernard and the statements of Moyse are correct. He enumerates the three cases of Bright, a case of Lloyd's, one of Elliotson's, one of Gould's (Anat. Museum of the Boston Society, 1847, p. 147), one of Susanna's (Giornale Veneto di Sc. Med. t. ii.), and then communicates one of his own. He then discusses the question rather briefly, and concludes that the presence of fat in the stools renders a functional disorder [illegible] pancreas probable, but not certain, and that, on the other hand, the abs[illegible]at from the stools does not negative the diagnosis of pancreatic other symptoms indicate it. We have discussed this point at such lately, that it is not necessary to consider this paper at length."

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* See the July number (1853) of this journal for a review on Pancreatic disease and fatty discharges.

V.—The Diseases of the Urinary Organs.

Change in the Urine, produced by the Inhalation of Arseniuretted Hydrogen. By Professor Vogel. (Archiv des Vereins. 1853. Heft 2. S. 208.)

Professor **Vogel** relates a case in which the inhalation of arseniuretted hydrogen gas by a gentleman who was experimenting with hydrogen, produced, in addition to other symptoms, a peculiar dark colour of the urine, which was found to depend on a large quantity of dissolved blood-colouring matter. In order to determine the cause of this, Vogel made many experiments on a dog, and found that the same effect was always produced by inhalation of the gas. The urine, of course, contained albumen at the same time, but no blood-corpuscles were visible. Vogel supposes that the gas acted destructively on the blood-corpuscles, and remarks that, in several diseases, as, for example, in typhus, there appears to be also a similar destruction and elimination of blood-pigment with the urine.

VI.—The Diseases of the Cutaneous System.

On Epiphytes of the Skin. By Dr. Gudden. (Vierordt's Archiv fur Phys.-Heilkunde. 1853. Heft 3. S. 496.)

Dr. **Gudden** communicates two papers on the diseases of the skin, produced by parasitic plants, and a third is promised. In neither paper is there any novelty, although the subject is well treated. As we reviewed Robins's complete treatise on parasitic vegetables in our last number, we shall pass these papers over. We may observe, however, that in order to see the plant in pityriasis versicolor, the author recommends a blister to be applied; the raised cuticle is taken, laid on a piece of glass, and the white layers infiltrated with serum are peeled off its under surface. Then nothing remains but a thin transparent layer, which permits the plant to be seen well; the plant grows most near the hairs and also freely on the borders of the sweat glands, but not in their interiors; it is contained in, and covered by, the most superficial layers of the cuticle, and forms two layers, the deeper being composed chiefly of thallus, the superficial of spores. The spores grow at the end of the thallus, and form clusters, which are so thickly set together that their mode of attachment to the thallus can only seldom be perceived; it is, however, by means of a little pedicle. In an appendix to his paper, Dr. Gudden communicates a case of partial alopecia of the hair on the pubis, which was found to depend on a fungus forming *round*, but not *in* the hair.

QUARTERLY REPORT ON SURGERY.

By John Marshall, F.R.C.S.

Assistant-Surgeon to University College Hospital.

I.—Injuries to the Chest and Abdomen.

1. *Impalement upon a Pichfork-handle entering per Vaginam.* By Dr. Sargent. (American Journal of Med. Sc. 1853, Oct., p. 355.)
2. *Extensive Laceration and Contusion.* By Dr. G. S. Bryant. (American Journal of Med. Sc. 1853, Oct., p. 399.)

1. The following case is recorded by Dr. **Sargent**. A lady, aged 37, slipped down upon a pitchfork-handle, which passed into the vagina 22 *inches*, and was supposed by Dr. Sargent to have gone upwards "between the uterus and the rectum, in front of the left kidney, behind the spleen, and between the diaphragm and false ribs, peeling

up the pleura till it reached the scaleni muscles." The first left rib was fractured, and emphysema was found above the left clavicle. The treatment consisted of rest, bandaging, and morphia. No pleurisy or pneumonia was detected! The recovery was rapid!

2. A case is related by Dr. G. S. Bryant. It occurred in the person of a negress, aged 25, who jumped down on to some fodder, in which was a tobacco-stick four-and-a-half feet long, and one inch square. The stick passed up the vagina, and on the right side of the os uteri, entered the abdomen, and finally lodged against the twelfth and eleventh ribs. There was considerable hæmorrhage from the vulva, and, subsequently, severe constitutional disturbance, with symptoms of peritonitis. On the fifth night after the injury, enormous hæmorrhage occurred from the bowels; the patient slowly rallied, being convalescent at the end of a fortnight, and perfectly well in a few weeks after.

II.—Aneurisms.

1. *Aneurismal Tumours upon the Ear, treated by the Ligature of both Carotids.* By Dr. Mussey. (American Journal of Med. Sc. Oct., p. 333.)

The American taste for journalism displays itself clearly in regard even to the medical profession; for their list of periodicals devoted to medicine and the collateral sciences is at least three times as long as ours. Nor can we hesitate to say that the national character for energy and activity is ably supported by our transatlantic brethren in surgery.

Dr. Mussey, of Cincinnati, records an interesting case of aneurismal tumours, situated on the external ear, treated apparently with success by ligature of both common carotids. The patient, a male, was 19 years of age. The tumours, of which three were on the left ear, and one below it, were as large as a grape or nutmeg, were soft, and compressible almost to obliteration; they pulsated strongly, and appeared to communicate like aneurismal varices. The integuments of the ear were hypertrophied. A cutaneous nævus had existed from birth in front of the left ear; but the tumours had first appeared as small elevations, only eight years since. One of the swellings had once burst, and caused alarming hæmorrhage, which recurred on removal of bandages and compresses. A farinaceous diet having been strictly enforced, the left carotid was first tied; the pulsation in the tumours ceased, and never returned; the tumours slowly reduced in size; but as it was doubtful whether a cure would result, the right carotid was tied four weeks after. The reduction in the size of the tumours was now more rapid. In about 3 weeks collodion was applied to the swellings with benefit, and 7 weeks after the second operation, scarcely a vestige of them was left. The patient was dismissed, and three months afterwards the cure appeared to be permanent. Contrary to Dr. Mussey's opinion, we should not feel quite confident of this.

III.—Tumours.

1. *Case of Fibrous Polypus of the Antrum, occurring in the Practice of Professor Syme.* By Dr. Dobie. (Monthly Journal, Oct., p. 307.)
2. *Exostosis of the Thigh Bone.* (Ibid., Nov., p. 420.)
3. *Cancerous Ulcer in the Temporal Region.* (Ibid., Oct., p. 306.)

A formidable example of fibrous polypus of the antrum, which occurred to Mr. Syme, and was treated by evulsion, is interesting, from the rarity of the disease, and the promptitude and success of the operation. The patient, a male, was 20 years of age; the disease first manifested itself by attacks of epistaxis, then by swellings of the right cheek, and partial obstruction of the right and left nostrils. Frequent and alarming bleedings accompanied the growth of the tumour. This, which was as large as an orange, was placed between the cheek and the jaw; it was lobulated, softer than an ordinary fibrous tumour, and seemed to grow from the antrum, but there was no bone over it. Provision having been made to plug the right nostril if required, access to the growth was obtained by making an incision from the angle of the mouth along the cheek, and the tumour, with some difficulty and delay, was torn out of its bed, within the antrum. One part of the mass reached backwards "between the pterygoid plates;" in detaching this, the internal maxillary artery was torn across, but speedily secured. The cure was rapid.

2. A curious instance is recorded by Dr. Dobie of the accidental fracture of the neck of an exostosis growing from the inner side of the thigh bone, a little above the knee-joint. The patient, a female, aged 23, had noticed a small lump for many years, in the situation of the tumour; its growth was slow, but it was about the size of an orange, when she fell from a ladder placed between a boat and a steamer, and struck herself in the situation of the growth. On examination, a large swelling was found a little above the knee, in which a hard movable body was felt. This was exposed by an incision, and easily removed, as it was quite detached. The case did well. The tumour was three inches long, and eight inches in its greatest circumference. The pedicle was bony, and measured two inches over at the fractured surface. The surface of the tumour was lobulated and cartilaginous; it was covered with a perfect synovial investment or bursa, a structure often before noticed by Mr. Syme in similar cases. A woodcut is given of this exostosis.

3. A case of cancerous ulcer situated on the temple is worthy of note, as well as another of the face, mentioned in the same report, from the mode of operation employed—namely, dissection of the diseased mass, instead of cauterization by chloride of zinc, or potassa fusa. Of the advantage of this plan we can venture to speak from actual experience. A case of cancer of the conjunctiva (Case 7), treated by frequent removal of the local disease without success, and afterwards requiring extirpation of the eyeball, is remarkable for the seat of the cancerous growth.

IV.—AMPUTATIONS AND RESECTIONS.

1. *Malignant Ulcer of Thigh—Amputation at Lesser Trochanter, occurring in the Practice of Professor Syme.* By Dr. Dobie. (Monthly Journal, Nov., p. 413.)
2. *Case of Cartilaginous Exostosis of the Condyle, Ramus, and Angle of the Lower Jaw, in which Resection was performed.* By Dr. Brainard. (American Journal of Medical Science, Oct., p. 397.)

1. A very satisfactory amputation of the thigh, immediately below the small trochanter, has been performed by Mr. Syme, for a malignant ulcer 8 inches long and 6 broad, situated on the inner surface of the upper part of the thigh, reaching to within 1½ inch of Poupart's ligament. The surface of the ulcer was elevated, and the discharge "muco-purulent," profuse, and acrid. It had commenced seven years before in one of a few small pimples on the skin. The patient's groins, lower part of abdomen, and right thigh, were deformed with extensive and prominent cicatrices, the result of burns received 21 years before. One of these crossed from thigh to thigh below the level of the symphisis pubis. The patient was 41 years old, and of a tolerably healthy appearance, but suffered much and severe pain. The glands in the groins were not (apparently) affected. Amputation being decided on in preference to excision, owing to the site of the ulcer, its proximity to the femoral artery, and the small chances of a good result, an anterior incision was made parallel to Poupart's ligament, from below the pubic symphisis to a point a little below the anterior superior spine of the ilium. A large outer and posterior flap was then made, the knife carried round the bone, and this latter sawn just below the lesser trochanter. Both the superficial and deep femoral

arteries were divided. With the exception of a slight tendency of the posterior flap to retract, reported on the 7th day, nothing occurred to interrupt the cure, which was complete in about 9 weeks. (Case xv.)

2. In the 'American Journal' will be found an account of an operation by Dr. D. BRAINARD, on a cartilaginous exostosis of the condyle, ramus, and angle of the lower jaw, in which resection, with removal of the zygomatic arch and the parotid gland, was performed. The operation would seem to have been performed with sufficient boldness, but the entire report is drawn up in so slovenly a style, that it defies the operation of resection on itself. The wound healed favourably, but left a sinus for many months; and the jaw fell so much to one side, that the patient became emaciated from the impediment to taking food.

V. AFFECTIONS OF THE GENITAL ORGANS.

1. *Obliteration of the Seminal Passages. Sterility consequent on Double Epididymitis.* By M. GOSSELIN. (Archives Générales de Médecine, Sept. 1853.)
2. *Case of Stricture of the Urethra, which had existed from early Infancy, cured by Operation.* By Dr. KELBURNE KING. (Monthly Journal, Sept. p. 201.)
3 *Two Cases of Stricture of the Urethra, treated by External Incision.* Reported by Dr. DOBIE from the Practice of Mr. SYME. (Monthly Journal, November. Case xvii., p. 416; and Case xxii., p. 421.)
4. *Recto-vaginal Fistula cured by Operation.* By R. D. MUSSEY, M.D. (American Journal of the Medical Sciences, Oct., p. 333.)

1. In a paper published in the 'Archives Générales de Médecine' (vol. xiv. and. xv., 4th series, 1847), M. GOSSELIN drew attention to certain facts which seemed to show that temporary or permanent obliteration of the vas deferens, at the tail or lower part of the epididymis, might possibly happen as a consequence of any kind of inflammation of those parts.

Since 1847, M. Gosselin has confirmed his views by other dissections upon the human body, and has further made some experiments on animals, as well as certain clinical observations upon patients suffering under the consequences of consecutive epididymitis.

In the cases of two dogs which were killed, one 10 months, and the other 4 months after, a portion of the vas deferens measuring 1 centimeter was cut out, he established the interesting fact, that such an operation is not necessarily followed (at any rate, within ten months) by any wasting of the testicle itself, although the vas deferens was quite interrupted. On the contrary, the gland presented its normal characters; nor was its secreting power altogether destroyed, for the flexuous canal of the epididymis was found distended by a fluid which contained a multitude of spermatozoa. It is not stated, however, whether these spermatozoa were active, but we are probably correct in inferring that they were. The remaining experiments on animals were fruitless.

The principal point in M. Gosselin's recent inquiries, and to which we now pass, consisted in the microscopic examination, at different intervals, of the semen of individuals, labouring under the effects of epididymitis, and having a more or less hardness opposite the lower part or tail of the epididymis.

Having justified the prosecution of such a subject out of regard to the interests of science, of individuals, and of families, the author preserves a discreet silence as to the mode in which the prosecution of his researches was rendered possible. Let us rather turn to his results.

Setting aside all cases of epididymitis occurring on one side only, and also examples of tuberculous or other organic disease of both testes, he refers to the results of the examination of 20 cases of double (bilateral) affection, consequent on gonorrhœa. In 15 of these cases, in which the epididymitis had lasted only a few weeks or months, the semen was examined from one to four times, at intervals of several weeks. They all agreed in three important particulars: 1stly, there was an induration opposite to the tail of each epididymis; 2ndly, the genital functions were seemingly restored in all respects, the semen appearing quite normal; but, 3rdly, no spermatozoa could, in the earlier periods of examination, be detected in that fluid. Pus-corpuscles (from the vesiculæ or the urethra), blood-corpuscles, molecular granules, and crystals of ammoniaco-magnesian phosphate, were occasionally found in it. Two cases only were quite followed out, and in these the return of spermatozoa in the semen occurred after some months, and coincidently with the complete disappearance of the induration of the epididymis on one side of the body. In one of these cases the right epididymis became affected in September, 1852; the left, two months later. On the 12th December the semen did not present any peculiarity to the naked eye, but contained no spermatozoa. On the 3rd January, 1853, the same conditions existed. The 16th May,—i.e., eight months after the first orchitis, and six after the second,—a few spermatozoa were found, and the induration on the right side was gone. On the 18th July, there were relatively more animalcules, and the left induration was only slightly evident. In the other case, the right orchitis took place in August, 1852; the left, in January, 1853. The semen examined on the 1st March, and again on the 5th April, was bloody, and contained no spermatozoa. These bodies were still absent on the 25th April and the 13th May; but on the 8th June they were again found, the left induration being effaced, whilst the right was still of the same size as before, and painful.

In the remaining 5 of the first-named list of 20 cases, the double epididymitis had taken place several years back. In one man, aged 45, the disease had occurred 20 years before, but the left induration no longer existed, and spermatozoa were found in the semen. This patient was at the time under mercurial treatment for secondary syphilis. In another man, the disease had lasted 5 years; the general health was good; but no spermatozoa could be detected. In the 3 other cases the duration of the disease was 10, 6, and 4 years respectively. There was hardness on both sides; the testes were otherwise unchanged, and free from pain, even after the act of emission; the signs and acts of virility were apparently perfect, and the semen had its ordinary appearances; the individuals were all married, but had no children; moreover, the semen was destitute of animalcules. One of them had had children by a former wife, before the attack of double epididymitis. In another, the semen had, some years before his attack, been examined, and found to contain spermatozoa.

From these details, which we have preferred to give at length, since no addition to the author's facts is likely to be made on this side of the Channel, M. Gosselin concludes, that in consequence of bilateral or double epididymitis, the ejaculated male fluid may cease for months, or even for years, or probably for the rest of life, to contain any spermatozoa, and may therefore be incapable of fecundating the ovum. Further, he believes that the absence of these spermatic animalcules is due to the obliteration of the seminal passages by "the effusion and organization of lymph around and within the flexuosities of the canal of the epididymis." To the objection offered by M. Rayer, that this effect might be owing to general debility, he replies, that the observations were made at periods when the patients had recovered their strength and apparent virility. That it is due to the effect of inflammation in the testicles themselves, he thinks unlikely, until it is shown that when the induration is gone, the spermatozoa are still absent. There appears, on the other

tozoa were found; hence M. Depaul's suggestion, that constitutional syphilis might have arrested the formation of the spermatic animalcules is also groundless.

In a physiological sense, M. Gosselin believes that his results show that Hunter's opinion as to the small relative proportion of the testicular fluid to that secreted by the vesiculæ seminales, is correct; and even that the quantity of the former is less than had been imagined. Hence the occurrence of copious ejaculations, even when the absence of spermatozoa indicated that no fluid came from the testes; and hence, also, the liability to obstruction, without pain, from the small *vis à tergo* of the secreted fluid.

Admitting the accuracy of M. Gosselin's facts, we must conclude with him that the following additions to the pathology of gonorrhœal epididymitis are henceforth necessary:—1. The obstacle to the passage of the semen is generally at the tail or lower part of the epididymis, but may be at some other point. 2. This obstruction is not accompanied by pain. 3. It produces no change in the condition or function of the organs appreciable by the patient. 4. When the obstruction exists on both sides, it must destroy the fecundating power; so, too, if on one side, if the other testicle be otherwise diseased, atrophied, or wanting. In one case of single epididymitis, and varicocele on the opposite side, no spermatozoa were detected. 5. The duration of the obstructed condition may vary. It may be removed any time before eight months, and possibly after a much longer interval.

In the way of treatment, M. Gosselin recommends very active measures, by leeches, and especially purgatives, repeated every three or four days, in the earlier stages of the attack. These are to be followed by mercurial inunctions, and particularly by the free use of iodide of potassium, which he believes, from observation, to possess peculiar properties of resolving chronic inflammatory deposits in the testicle, even those which accompany tubercle or cancer. In gonorrhœal epididymitis it is very efficacious, especially when given early. The dose to be employed is not to exceed 1 gramme, equal to about 15½ grains.

2. The case here to be considered was under the treatment of Dr KELBURNE KING, of Hull, and well exemplifies the value of the urethral section in an old, obstinate, impenetrable, and complicated stricture.

The patient (aged 33 at the time of the operation) had suffered from urinary irritation before he was old enough to express his sensations; at school he had always had slow and painful micturition. At 7 or 8 years he was sounded with great difficulty, for stone, but nothing was detected. From that time until his 12th year, small bougies were at intervals passed through the stricture; but after that period no instrument could be introduced into the bladder. Frequent, imperfect, and painful micturition were constant symptoms, but no blood had ever passed. At the age of 24, owing to more serious suffering and threatened retention, an attempt was made to force the stricture, without success, but with the ultimate result of causing perineal abscess and fistulæ. From the above-named period he became worse, and had continual incontinence of urine, with painful paroxysms of retention. In September, 1851, the patient, being then 32 years of age, came under Dr. King's care. Several fistulæ existed on the scrotum, and a hard knot could be felt at the seat of the stricture, midway between the pendulous portion and the bulb. The smallest sized bougie was arrested at four and a half inches from the orifice of the urethra, and the stricture could not be overcome. The urine was muco-purulent, and easily became alkaline. After continued suffering for some months, of a most severe kind, and having a serious effect on his health, another abscess formed in the perinæum, and when this was opened, the patient being under the influence of chloroform, an attempt was made to pass a bougie, by the aid of the finger, in the wound. The attempt failed, but a small calculus, about the size of a pea, was removed from the bottom of the abscess. The urine now flowed partly through the wound and partly by the natural passage, and of course considerable relief followed. But no instrument could even now be passed into the bladder; micturition was frequent, and accompanied with much straining. The stream was small, and mostly in drops, much passing by the wound. He had no power of propulsion, and the urine still threw down a copious muco-purulent deposit.

In October, 1852, a small grooved staff was passed as far as possible, getting it fairly into the stricture, an incision being then made through the skin, in *front* of the *scrotum*, and the induration felt with the finger; the urethra was opened in front of the stricture, and the knife pushed on through the firm substance to the end of the groove, where it struck against a calculus, which was immediately cut down upon, and the "four or five" calculi, weighing altogether ten grains, were turned out of a membranous sac. The staff, which had been held in its place, was then easily passed on into the bladder. A No. 6 catheter was afterwards introduced, and retained. The case followed in the ordinary way. In a month's time only a few drops of urine passed by the wound; No. 9 catheter was easily introduced, and the patient could propel from him a full stream of urine, which he had not done from his early days. His general health became quite re-established. Dr. King informs us that the fistulæ all healed, and the wound gradually closed. A bougie was passed occasionally.

Dr. King discusses, at some length, the question, whether this was a case of early stricture, subsequently complicated by the calculi? or whether it originated in the formation of a calculus in early infancy or youth, and its subsequent imposition in the urethra? The former view is adopted first, because the symptoms were developed gradually, and not suddenly, and there was no blood in the urine; secondly, because instruments were passed with difficulty up to the age of 12 years, and no calculus had been felt then; thirdly, because the calculi were so small; and fourthly, because there was no occurrence of symptoms, either local or general, at any time, indicating the descent and impaction of the stones. If this view be correct, the case is a very rare one, but the author quotes another, occurring at four years of age, recorded by John Hunter. Certainly the *practical* impermeability of the stricture—an interesting fact—seems to have been due to the thickening and distortion of the canal produced by the local detention of the calculi within it.

The causes of so early a development of stricture are quite unknown.

It is not our intention to follow Dr. King in the discussion of a case of stricture, in which the urethral section was rendered a second time necessary, and which was treated in succession by Mr. Fergusson and Mr. Gay; nor do we here intend to examine minutely the merits of an operation which has undoubtedly been eminently successful.

In reference to the operation itself, it has only to be remarked, that as the grooved staff could not be got through the stricture, the next best course—viz., that of getting it as far as possible—was adopted. We must observe, also, that the section was made through "the front of the scrotum."

3. Two cases (xvii. and xxii.) of urethral section are recorded by Dr. Dobie. The first presents no very remarkable features; in the second case the seat of stricture was only one inch from the orifice of the urethra, and the narrowing was very great. An external incision, upon a grooved direction, was made, and a catheter retained for thirty-six hours. The wound healed on the fourth day.

4. Dr. Mussey, of Cincinnati, relates a successfully treated case of recto-vaginal fistula, large enough to admit two fingers. The sphincter having been divided purposely some days before, so as to facilitate the contraction of the fistula, the edges of this latter were pared and brought together by the clamp suture of Dr. Sims, secured by the wires and split shot. The principal feature in the subsequent treatment was the management of the diet, which consisted, for eighteen days, of two, or two and a half *crackers* (biscuits) a day—i.e., under five ounces of solid food. On the eighteenth day a gill of milk was allowed. An elastic catheter, removed and cleaned every second or third day, was kept in the bladder until the eighteenth day. On the seventh day the wires were cut, and the clamp was removed; the wound was united from end to end. On the twenty-fourth day the bowels acted for the first time, from the use of castor oil. More food was then taken, and the patient left the hospital in the fifth week after the operation.

In commenting on this case, Dr. Mussey remarks that he prefers to use a thicker cylinder or clamp than Dr. Sims—viz., one and a half line diameter instead of one line; and to place his wires nearer—viz., one fifth of an inch apart, instead of one third. Moreover, he employs wire twice the thickness of a horse-hair, instead of that of equal thickness.

VI.—AFFECTIONS OF THE RECTUM.

Cause and Treatment of Prolapsus of the Rectum. By M. DUCHAUSSAY (Archives Générales de Med., Sept.)

In a short but interesting memoir, M. DUCHAUSSAY reviews the circumstances attending this troublesome complaint, and fixes attention in particular upon the loss of power in the sphincter ani muscle as the chief cause of the descent of the bowel. Moreover, he endeavours to show that Dupuytren's operation, by excising the radiating folds of skin around the anus, and the operation by four touches with the actual cautery, practised by Guersant, act not by causing any subsequent retraction of the cellular tissue, skin, and mucous membrane, but rather by stimulating the sphincter muscle, so that it regains its contractility, and therefore its retentive character. How else, asks M. Duchaussay, do we explain the fact, that the prolapsus is often cured, or does not return after two days, or even after one day, or not at all, after the operation? He points out the fact, that in cases of this disease in infants, three fingers may sometimes be introduced without causing contraction of the sphincter, before the operation by cautery, whilst afterwards, if one be passed, a powerful contraction of the sphincter immediately ensues. As proof that this recovery of contractile power by the sphincter is the cause of cure, a case is mentioned in which M. Guersant had used the cautery too superficially, the sphincter failed to contract, and the disease returned. A second cauterization was followed, on the contrary, by return of the muscular contractility, and the cure was complete.

According to the author, the cautery acts as a stimulant to the paralyzed muscle, just as it will to the deltoid in a like condition. After pointing out the inconveniences and apparent severity of M. Guersant's method, M. Duchaussay suggests that a slighter cautery, or some other stimulant to muscular contractility, might act as well; and he suggests strychnine. This, with M. Guersant's permission, has been tried in the Hôpital des Enfants, in the case of a girl aged eleven years. The prolapsus here arose from obstinate constipation; it had lasted for four years; the bowel protruded at each evacuation about ten centimeters (= 4 inches). During the first month of her admission she was treated by laxatives only, with no other result than that of diminishing the length of the protruded portion of bowel to about four centimeters (1½ inches). Strychnia was then employed endermically near the region of the sphincter; the next day there was no evacuation; on the following day the bowels acted once, only a slight bulging of the rectum taking place; on the third day the protrusion was still less after an ordinary evacuation; and during the next thirteen days it did not occur again.

Blisters were made in the cleft between the nates, and on the right thigh close to that cleft; one-sixth of a grain of strychnia was applied the first day, one-third on the second, and one-third on the fourth day. On the fifth day, about half a grain of sulphate of strychnia was used, and this was repeated for the last time on the sixth day. In the case of a boy, it is recommended to be applied between the scrotum and anus, immediately over the anterior interlacement of the sphincter ani fibres. The remedy certainly deserves further trial.

QUARTERLY REPORT ON FORENSIC MEDICINE, TOXICOLOGY, &c.

By W. B. KESTEVEN, M.R.C.S.

MEDICAL Jurisprudence in Great Britain has no distinct organ whereby to vindicate its condition and progress. In Paris, in Berlin, and in Vienna, journals devoted thereto preserve all the most important facts relating to continental forensic medicine. On this side of the Channel these topics must be sought throughout professional journals, or ordinary newspaper reports of trials, coroners' inquests, &c. It is not our object to trace the cause of this blank in British